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Philosophy of Science

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Is Scientific Verification Possible in Philosophy?

BY

C. J. DUCASSE

1. *The present state of philosophy contrasted with that of the sciences.* Both philosophy and science seek not mere opinion but knowledge. The sciences, however, have by now won a vast body of knowledge, and daily make positive additions to it notwithstanding their theoretical controversies. In philosophy, on the contrary, the same great problems are discussed by generation after generation with rather meager results other than a multiplication of theories and schools of opinion. One is therefore moved to wonder whether philosophy is forever doomed to inconclusiveness, or whether on the contrary knowledge and definite progress are possible in its field in the same sense as in the sciences.

2. *The typical knowledge-yielding method.* This question leads one to ask in turn what has rendered possible the immense additions to knowledge made by the sciences. And the answer is undoubtedly that the sciences owe their progress chiefly to their mastery and use of the method which Jevons proposed to call the Newtonian method, namely, the method of Hypothesis-Deduction-Verification.

As we know, two sorts of tests are applicable to a hypothesis. One is to ask whether empirically observable facts verify it; and

the other is to ask whether the given hypothesis is consistent with other hypotheses related to it. But this consistency test can yield no evidence of the truth or probability of a hypothesis unless some at least of the other hypotheses with which it is consistent have themselves met the other test, namely that of verification by observable facts. For so long as mutual consistency of hypotheses is the only test applied, it remains possible to construct as many sets of hypotheses as one pleases,—each set internally coherent, yet each different from every other. And for a choice between the various sets, the consistency test furnishes no basis whatever. Only empirical verification decides between them. And one of the chief reasons why philosophy has made little positive progress as compared with the sciences is that, although it has used hypotheses, and has applied to them the test of mutual consistency, it has not adequately understood in what consists the process of empirical verification when philosophical hypotheses are concerned. The problem whether, or how, scientific verification is possible in philosophy therefore essentially reduces to that of pointing out the nature of the facts and of the process by which philosophical hypotheses can be empirically verified.

3. *What form the solutions of typical philosophical problems would take.* Some typical philosophical problems would be for instance, What is Truth? What is Reality? What are Knowledge, Mental states, Objects, Objective perception, Introspection, Right and Wrong, Beauty, Probability, Relevance, etc. To avoid in what follows the cumbersomeness of having to recite such a sample list whenever we wish to refer to typical philosophical problems, let us pick one at random from the list,—for instance, What is Introspection as distinguished from so-called external Perception,—and let us phrase the remainder of our discussion in terms of that example only,—being cautious however, not to say concerning it anything that could not similarly be said concerning any of the other problems.

This being agreed, let us note first that an answer to the question "What is Introspection?" would take the form of a definition, no matter how complex, of the term Introspection. But obviously, if that definition were a merely verbal definition, it would

settle no disputes. To do this, it has to be on the contrary a so-called *real* definition,—one, namely, stating the essential nature of the actual process itself to which the name of Introspection is applied. For a real definition differs from a merely verbal in that, in the case of real definition, the term defined already possesses a certain application; whereas in the case of verbal definition the question what, if anything at all, the term defined applies to can be answered only by a subsequent empirical search.

The definition of Introspection sought must therefore include no character that turns out to be absent in some of the concrete cases of observation to which the name of Introspection actually is applied. And this evidently provides an empirical test of the validity of any definition of Introspection that may be proposed.

4. *Identification of concrete cases of Introspection whereby to test proposed definitions of the term.* We seem, however, to be here in a predicament, for if we do not already know what characters the term Introspection connotes, how can we then identify the concrete cases to which the name applies? And if we cannot identify them, how then can we appeal to them to verify or invalidate some proposed definition of Introspection? This predicament might easily delude us into believing that there can be no empirical test of the validity of a proposed definition of Introspection, and that the only test available therefore is that of consistency of the proposed definition with the definitions of related other terms. But we have already seen that we could construct as many alternative sets of such definitions as we pleased, and that for a choice between them consistency would furnish no basis.

5. *Intuitive vs. Discursive Knowledge of what a word means.* The predicament, fortunately is only a spurious one. This becomes evident as soon as we notice that there are two sorts of knowledge of what a word means,—one, intuitive only and habit-generated; and the other discursive and obtained as a result of analysis. Discursive Knowledge of what a word means is hard to get, and we have it of only a few of the words of the language. It permits us to draw remote yet accurate inferences from the application of a given word to something, and it also enables us to decide in any doubtful case whether the word is or is not applic-

able to a given thing. Evidently, it is knowledge of this sort that we now seek concerning the meaning of the term Introspection.

Intuitive, habit-generated knowledge of the meaning of words, on the other hand is easy to get and is the sort everyone has of all the words of a familiar language. Such merely intuitive habitual knowledge of the meaning of a word is too indefinite to furnish a basis for accurate remote inferences,[1] and too indefinite also to enable us to decide as to the applicability of a word to a thing in any case other than the familiar ones from which we acquired our intuition of the word's meaning.

But in any such familiar case that intuition *is* fully adequate to identify the case as one to which the word does apply, or as the case may be, does not apply. And this is the escape from the predicament mentioned. For obviously, a word applies to certain things and not to others, ultimately for no other reason than that people actually use just that word as the name of just those things. This is evident in the case of proper names, but it is no less true in that of the names of Kinds. For example, the only ground there is ultimately for asserting that our understanding of the word "spoon" is really an understanding of what spoons rather than perhaps forks are, is that the objects by observation of which we learned the meaning of the word "spoon" were the ones to which that word and not the word "fork" was actually applied by everybody. And if, for the purposes served by a spoon, an object is indistinguishable from those called "spoons" by the generation that taught the language to ours, then the intuition we have of the meaning of that word adequately identifies the object as being one of those called spoons. Doubts or disputes arise only when the object not only resembles but also noticeably differs from the familiar ones to which we were taught to apply a word. For example, in the minds of children familiar with spoons but not yet with shovels, there might be doubt, on perceiving a shovel, whether or not it is a large spoon.

6. *Selection and nature of the undisputed facts by which to test*

[1] It is because philosophers have had only such intuitive knowledge of the meaning of many of the terms they employed, that the inferences they have drawn from the application of those terms have so often been loose and inconclusive.

disputed definitions. In the light of these remarks it is obvious that however numerous may be the disputed applications of the word Introspection, any dispute as to its definition is merely verbal unless the word has *some* undisputed applications and inapplications. And a sample list of each of these need not be particularly hard to assemble, especially if the disputants realize that the cases they propose for the lists should be picked intuitively rather than by means of one or another of the very definitions of Introspection to be tested by the listed cases.

But what most needs now to be clearly realized is the exact nature of the items that will make up the two lists. Each item will consist of an admittedly true or admittedly false applicative assertion; namely, of an assertion of the form: "The species of observation of which so and so is a case is called Introspection." Examples of items belonging to the list of admittedly *true* applicative assertions might be such as the following:

The mode of observation we use to answer the question "Do you feel any pain?" is called Introspection.

The mode of observation we use to answer the question "Do you like that man?" is called Introspection.

The mode of observation we use to answer the question "Do you prefer this color to that?" is called Introspection.

And examples belonging to the list of admittedly *false* applicative assertions might be such as the following:

The mode of observation we use to answer the question: "Is that tree an oak?" is called Introspection.

The mode of observation we use to answer the question: "Are these sheets of paper clipped together?" is called Introspection.

The mode of observation we use to answer the question: "Is this material combustible?" is called Introspection.

Beside the two lists of admittedly true and admittedly false applications of the word there will of course be a third list, containing all its doubtful or disputed applications.

7. *Verification of a proposed real definition.* We are now in position to say just how the method of Hypothesis-Deduction-Verification is applicable to such a philosophical problem as that of the distinction between Introspection and so-called external

perception. The *hypothesis* will consist of some proposed definition of Introspection, or, since Introspection is admittedly a species of the genus Observation,—the hypothesis will consist more particularly of some conjecture as to what differentiates Introspection from so-called external perception. The *facts* by which the adequacy of the hypothesis is to be tested will consist of the two lists of admittedly true and admittedly false assertions applicative of the word Introspection. The *deduction* from the hypothesis will consist in *replacing the word Introspection by the proposed definition of it*, in each of the assertions of the two lists. And the *test of the adequacy* of the hypothesis will be *whether this replacement in any of the admittedly true* (*applicative*) *assertions yields an observably true* (*descriptive*) *assertion; and in any of the admittedly false* (*applicative*) *assertions yields an observably false* (*descriptive*) *assertion.*

If no assertion that fails to meet this test is found in either of the two lists, then the definition is rightly held to have been verified by observation of the admitted relevant facts so far as observation of them has gone. That is to say, the proposed way of defining the word is then known to be so far consistent with the admitted applications and inapplications of the word. But, as with the hypotheses of the sciences, a single clear-cut case where the hypothesis conflicts with the admitted observable facts suffices to require a modification of the hypothesis; and, on the other hand, no number of observed facts with which the hypothesis agrees completely prove it,—although the more numerous and diverse the cases of agreement, the more probable becomes the hypothesis.

8. *A definition so tested has the authority needed to settle disputes.* An originally disputed definition which is verified by the test just described is thus given a positive probability by observation of the facts admitted by all parties to the dispute. Such a definition is then no longer open to the charge of arbitrariness. On the contrary, the very fact that it is supported by observation of the admitted evidence confers upon it the authority needed to decide whether the word defined is or is not applicable to some of the until then disputed or doubtful cases; and also to decide whether

certain until then disputed inferences about a thing could or could not be based upon the supposition that the word is applicable to the thing.

It is worth nothing that the empirical test described can be used not only to settle such disputes, but also to ascertain whether a philosopher, who has stated that by a certain word he will mean so and so, actually applies the word in a way consistent with his announced definition. For if it is possible to find anything to which he does apply the word, but which yet lacks some character included in his definition; or anything to which he refuses to apply the word, but which yet has all those characters, then,—whatever the consequences to his philosophy, he must modify either his application of the word, or his definition of it.

9. *The coining of technical terms.* Except in this last remark, the present paper has considered only such philosophical terms as already are an intrinsic part of the language. Concerning philosophical terms which on the contrary are technical terms specially coined by a writer for his needs, it is possible on the present occasion only to remark that,—however free a philosopher may seem to define his technical terms as he pleases,—nevertheless those technical terms sooner or later enter into statements which are directly or indirectly about admitted facts of the type already described. And therefore no term entering into a statement about such facts can be assigned by definition a meaning incompatible with what empirical observation of those facts would reveal. For to divorce the definition of a word from the applications made, or to be made, of the word would be only to amuse oneself with arbitrary, purely verbal definitions.

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The Nature of the 'Given'[1]

BY

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IT IS by no means impossible that the discussion of epistemological problems, at the present stage of our understanding of the issues involved, may be futile. But so long as anyone desires to "know" anything at all, just so long it will be impossible for thoughtful men to leave the questions of epistemology alone. So, futile or not, this paper is concerned with an analysis of one of the most fundamental of epistemological problems, namely that of the 'given.' It is true, the 'given' has recently received an unusual amount of attention from philosophers. What with two of the American Philosophical Association's presidential addresses last year having much to say on the subject, and what with the logical positivists making the 'given' the cap-stone of their radical empiricism? All of which, it will be claimed, proves the contention that any attack upon the problems of epistemology, from the standpoint of our present knowledge and understanding of the total situation involved, is condemned to hopeless futility. Why, then, add one more hopelessly futile attempt to those of all the rest? If, despite such initial misgivings, I shall proceed to my own analysis of the 'given,' I shall do so, first of all, in recognition of the indisputable fact that everyone of us, even though we should be blind, deaf and dumb, do, as a matter fact, find ourselves confronted with something 'given'; and, secondly, because it appears to me that, as philosophers, it is still our task seriously to concern our-

[1] Read in substance before the Pacific Division of the American Philosophical Association, Berkeley, California, December 27, 1934.

selves with unsolved problems, even though they may not yet be capable of being so stated that a unanimously acceptable solution for them may be—at present—possible. Even the logical positivists, who, of course, are most critical of what philosophy is or ought to be, insist that "the object of philosophy is the logical clarification of thoughts."[2] Since the 'given' is a concept (thought) most widely used not only by so-called 'pseudo-philosophers' but even by the logical positivists themselves, it certainly cannot be denied that the clarification of the concept of the 'given' is decidedly a philosophical task. More than that: it is a *necessary* philosophical task just so long as there is such wide difference of opinion concerning the 'given' among philosophers.

Of course, the logical positivists themselves—despite their definition of the nature of philosophy—do *not* find it necessary to analyze further the nature of the 'given.' For, according to them, the 'given' is a no-further-analyzable finality. You can point at it, but you can say nothing more about it. The 'given' is the point where the logical positivists turn mystics. The 'given' is just that—*und damit basta*!

It might, of course, be very nice if this were so; but, unfortunately for the logical positivists, it is not so. It is one thing for a mule simply to lie down and refuse to budge. It is a quite different thing to prove that nothing further *can* be said or done about it. As at so many other points, the position of the logical positivists concerning the nature of the 'given' is a combination of *ex cathedra* dogmatism and philosophical over-simplification. The aspect of truth contained in the positivistic insistence at this point lies, of course, in the statement that any specifically "given something" can not be reduced to anything further back of it. In this—very specific—sense the 'given' might be granted to be a finality. But this is far from the equivalent of saying that the nature or essence of 'given-ness' can not be described or further analyzed. It certainly must be possible to characterize the nature of 'given-ness' sufficiently for us to be able to tell what we mean by it—besides merely pointing at instances of it and saying: "that." For it is precisely the fact that when we want to know

[2] The object of philosophy as stated by Ludwig Wittgenstein in his *Tractatus Logico-Philosophicus*.

the *nature* of the 'given,' we are *not* concerned with any *one* specifically given something, whether that something be the red lamp-shade or the distant star. If we ask: What do we mean by the 'given'?, we are *not* asking: What is it that is *now* given to me?, nor are we asking: What is *ever* given? The answer to this last question would be, obviously, an infinite number of things. Rather we are asking: Wherein is all this infinite number of things—no matter what—alike *qua* given? To this question a meaningful answer *must* be possible or else we ought to cease to use the *word* as well as the *idea*.

Before undertaking to give a positive and systematic answer to this question, let me clear the ground somewhat by pointing out the untenability of several positions which philosophers have quite commonly taken with reference to the nature of the 'given.'

Idealists and realists alike have erred seriously in their treatment of the 'given,' though—of course—at opposite ends of the same fallacious way of dealing with the problem. The neo-realists, especially, seem to have felt it to be their duty—apparently for no better reason than for the fact that they are genuine realists!—to declare the nature of the 'given' to be wholly independent of any receiver or of any receiving organism. They seem to feel that the independent reality of a realistic world can be saved only by the denial of any presence or representation of any receiving consciousness in the being of the 'given.' In doing so they have, obviously, interjected *metaphysical* questions and considerations into a fundamentally *epistemological* problem. For, the disclaimer appears to be concerned with the ontological status of the 'given'; while the epistemological problem of the 'given' concerns itself purely with the question: *How*, precisely, is anything 'given'?—no matter what its ontological status may be. The fact of the case is that, when we are searching for the nature of the 'given' in the cognitive process, we are *not* discussing—or even as such interested in—the question of the metaphysical status of the 'something' which is 'given.' The latter question is an entirely separate problem, which, when mistakenly brought into the epistemological problem under consideration, can do

nothing but cause endless confusion. Such confusion invariably results when problems of inherently different nature and character are mixed in such a way as to conceal their fundamental differences. In passing I may remark that I think the century and a half which have passed since Kant's epoch-making first *Critique* have served to emphasize the indisputable validity of his claim that, from the standpoint of any verifiable *knowledge*, nothing whatever can be said about the ultimate nature of the *Ding an sich*. This need not necessarily be taken to close the door upon metaphysical speculation. But such speculation should recognize itself as what it is, namely *speculation*, and not confuse itself with knowledge. Be that as it may, things 'given' are experiential and verifiable facts[3] of every day human experience; and the nature of such given-ness should be studied and analyzed as such, wholly irrespective of the speculations concerning the ultimate metaphysical character of the things that are 'given.'[4]

I would, therefore, call upon the neo-realist to be *more* realistic, that is to say, more true to the every day facts of the case. To be afraid of elements which are, obviously, present in every instance of 'given-ness'—just because the admission of such elements (in this case the receiving consciousness and organism) would *seem* to commit one to a denial of the independently existing reality of the so-called external world—is certainly to be something other than "realistic."

But the idealists have blundered just as grievously. In order

[3] I am, for the sake of brevity in the present discussion, using the word "facts" in the ordinary sense of every day language, though of course fully aware of the problematic character of facts themselves. But a treatment of the nature and character of "facts" (their flexibility and chosen and interpreted nature) deserves a separate paper, which cannot here be interjected. The interested reader may, however, be referred to the volume on "Facts" of the *University of California Publications in Philosophy* (vol. 14, 1932), in which varied and able discussions of this problem may be found.

[4] This must not be taken to mean that I assume it possible to approach such analysis with a wholly "blank mind." No conscious adult mind can approach *anything* in such fashion, as I have tried to show elsewhere (cf. my essay "Is 'Standpointless Philosophy' Possible?," *The Philosophical Review*, vol. XLIV, No. 3 (May 1935)). But it is one thing to admit the necessary presence of the background of one's own life and experience, and a quite different thing to insist upon approaching everything with an already completely determined (speculative) metaphysics.

to save the reality and creative power of the mind, they seem to have felt called upon to deny the reality of anything existing independently of such consciousness. Here too the error is largely one of making a metaphysical issue out of an epistemological one.

After all, as I have already tried to point out, it is not necessary to decide any questions of the ultimate nature of Reality in order for us to be able to analyze what takes place in the cognitive process when a knowing mind finds itself confronted with something which neither is a part of the knowing mind nor has been brought into existence by this mind; which, moreover, is whatever it is, quite independent of that mind.

I think it is not only possible, I think it is absolutely *necessary* for us to deal with these epistemological questions quite irrespective of any metaphysical faith or speculation. I hasten to add that I am not afraid of metaphysics; nor do I for one moment agree with the logical positivists who claim that they find all metaphysical questions wholly meaningless. I think that both Professor C. I. Lewis and Dr. Donald C. Williams have quite effectively and irrefutably answered those noisy claims.[5] But it seems to me always to be ineffective, not to say dangerous, needlessly to mix problems which lie in quite different fields.

Thus it is *one* thing to try to determine what, precisely, the nature of external reality is—in terms of metaphysical speculation. But it is quite *another* matter to ask *how* is anything, which is given to a sensing being, 'given?' This latter question should be capable of being answered from a factual analysis of the actual processes of all the factors which can be found to be involved—quite irrespective of what one may finally speculatively decide the nature of ultimate Reality to be.

In the light of the discussion thus far, let me summarize my negative conclusions concerning the nature of the 'given.' The 'given' is

(1) neither an independently existing reality, that is to say,

[5] Cf. C. I. Lewis, "Experience and Meaning," *The Philosophical Review*, vol. XLIII, No. 2, (March 1934), pp. 125–146, and Donald C. Williams, "The *A Priori* Argument for Subjectivism," *Monist*, July 1933, especially footnote 34 on page 193.

something which—*qua* 'given'—exists wholly independent of any organism to which it is given,

(2) nor is it wholly the creation of the receiving organism (or knowing mind) independent of anything outside of that organism,

(3) nor is it the "agreement" between what is independently given and the creative activity of the receiving organism (or knowing mind).

Each one of these erroneous interpretations of the nature of the 'given' lead to absurd conclusions. In the first instance, the very 'given-ness' of the 'given' is denied in order to save the independent reality of the outside world. Thus the outside world is saved; but the 'given,' so far from having been explained, has been annihilated. The external world, that is to say, exists, but it is *not* given. It certainly is strange how such an interpretation could still lay claim to be an *explanation* of the 'given.'

In the second instance the 'given-ness' is again explained away, but this time in favor of the asserted singular existence of Mind and of its creative processes. Thus Mind is saved, but the external world disappears. And again, nothing is 'given' to the receiving organism, but the organism itself creates whatever it ever possesses.

In the third instance also the 'given' as *one* event[6] disappears in favor of two, or, to be more precise, three events; viz., (a) an independently given external something, (b) the creative activity of the receiving organism (or mind), and (c) the supposed "agreement" between a and b.

Each one of these profferred solutions of the problem of the 'given,' therefore, need to be rejected because, so far from *explaining* the 'given,' they simply explain it *away*. Each of these attempts leaves the nature of the 'given' wholly unsolved.

How, then, is anything given?

In order to answer this question let us proceed with an analysis

[6] It should be unnecessary to prove at length that anything 'given' in experience is just such a *one* event in ordinary experience, though what is 'given' may be ever so complex. The one-event character of anything 'given' has nothing to do with the simplicity or complexity of what is 'given.' This involves another problem which cannot be discussed here.

of a 'given' something, such, for example, as the typewriter on which these lines are being written or—if that should be considered to offer a particularly difficult example of something 'given'—the planet Venus.

I suppose all philosophers, of whatever philosophical persuasion, would count *both* objects, especially if both are immediately present to the perceiving organism, as 'given' somethings. (It must be kept in mind, however, that *what* those somethings are ultimately—conceived *not* as 'given,' but wholly independently of their being 'given'—does not in the least concern us here. That is a metaphysical, not an epistemological problem.) Nor can a factual analysis of the 'given' as such be concerned with the question as to how anything, such as the planet Venus, can be *immediately* 'given' to me *now* in my present perceptive processes when I know all the time (from scientific analysis) that the planet itself is not at all immediately present to me now, but that only the light from it—having left the heavenly body some time ago—is now visible and as such immediately 'given' to me. I do not deny that such considerations may be interesting and may be both philosophically and scientifically important in their own right. But I must insist that they have nothing factually or analytically to do with the problem of the nature of the 'given.' For even in the case of the star-light coming from a distant planet, something external to the perceiving organism *is directly* and *immediately* 'given' to the perceiving organism just as surely as something is immediately 'given' to the organism when I behold the typewriter which I am using. All of which seems to me to be quite obvious.

A very little further analysis of this same factual experience reveals as equally obvious that neither the typewriter nor the planet Venus are the *ad hoc* creations of my own mind or organism. For, strange as it may seem, now that I am in their perceived presence, yet I know it to have been true of much of my previous experience, that there have been numberless times when—though my eyes were equally as open as they are now and my eyesight was no more impaired than it is now—I did *not* find myself immediately confronted either with my typewriter or with

the planet Venus. More than that: there have been times when I very much *wanted* and needed my typewriter, but when all of my thinking of it and all my recalling it and all my desiring it did *not* bring the typewriter so before my sensory perception that I could use it for the purposes for which I needed it. No amount of mental creativeness or memory could actually *produce* the 'given' typewriter for my immediate purposes. Typewriters seem to have a way of existing by themselves. When, thus existent in their own right, they come into our immediate experience we can use them for our own purposes. But no amount of *mere* thought *about* them can bring them into useful existence.

Nor does the matter stand differently with the planet Venus. Many have been the nights when we may have looked into the sky to locate the planet, only to find ourselves balked in the attempt by not only not being able to see Venus but no other celestial body either. Cloudy, rainy nights have wiped out every star in the sky—so far as their given-ness to us in immediate perception was concerned. The light from Venus had to travel just as far on such nights as on any other to reach our eyes. Nor is there any philosopher anywhere on earth who really would care seriously to argue that, on such nights, neither Venus nor any other star existed anywhere in the universe. But certain it is that, on such nights, no amount of thinking about stars has been able to present us with the immediate given-ness of Venus in visual perception.

It is clear, then, that the actual things with which we find ourselves confronted when anything is 'given' to us in sensory awareness—whatever else they may or may not be—are *not* the pure and sole creations of our own minds or of our private organisms. As indicated above when talking about type-writers, these things have a way of existing whether we will or no. Often they stand before us wholly without any volition on our own part or even confront us very definitely against our desire. Their *existence* does *not*, therefore, depend upon either our perceiving, or volitional, or mentally creative activity. They can, yes, they do exist quite independent of our perceiving or knowing activity. This much even the most acute philosophical analysis would

appear to me to have to grant to what may truly be called the realism of common sense.

The—admitted—fact that we cannot tell *what* any of those things are independent of our knowing minds, is no argument whatever against the absolutely necessary[7] admission on our part of their existence wholly independent of our perceiving or knowing organism. The only thing which this admitted fact can be made to prove is that we can only perceive and know things *qua* perceived and known, that is to say, *through* the only avenues of perception and of knowledge which we possess. If anyone can get any comfort out of such admission, he is welcome to it. To me it appears to be either tautologous or platitudinous. For it really says nothing more or less than that we perceive things as we perceive them and that we know things as we know them. Sure enough; but, what of it?

This much at any rate: that, although it is positively absurd to deny the existence of objectively independent real things, it is equally absurd to attempt to account for anything 'given' without recognizing the fact that *qua* 'given' it is *not* just the independently existing real thing wholly undetermined by the nature of the organism to which it is 'given.' *Qua 'given' anything is what it is because of its own inherent nature* (whatever that may be, though perhaps totally unknown to us) *plus the way in which it has to appear to the organism* (or mind) *to which it is 'given' because of the nature of the receiving apparatus which this organism possesses.* To assume or describe less than this when describing anything 'given' is to be false to at least some of the discoverable and analyzable facts which are clearly traceable in anything 'given' to us through our senses.[8]

To jump from the admission that, *qua* 'given', everything is what it is not only in virtue of its own inherent nature but also in virtue of the nature of the receiving apparatus of the receiving organism—to jump from this to the assertion that therefore things

[7] Absolutely necessary, that is to say, from the point of view of a common sense interpretation of the facts of every day experience.

[8] And it is fundamentally such 'given-ness' through the senses that the logical positivists have been using as their no-further-definable or -describable finality.

have no objective existence independent of the perceiving organism (or knowing mind) is a wholly unwarranted leap. If such a leap is made by way of one's personal pre-dilection or metaphysical speculation, that is one thing. But to say that a factual analysis of the nature of the 'given' necessitates such a leap is wholly untrue.

Nor does it amount to anything very rational to assert that, in the light of our analysis of the nature of the 'given,' we can never know Reality but only Appearance; that, at best, what is 'given' to us is only Reality-as-it-appears to such perceiving organisms and minds as we are. To make that kind of a rejoinder amounts to nothing more or less than a desire on the part of the arguer to know Reality without knowing it. Such a desire can hardly be called rational. Knowledge and the knowing process happen to be what they are. To ask something totally different from them is—to my mind—simply to make ourselves absurd. It is obvious that we can know Reality (or anything else, for that matter) only as we can know it. That is to say, human knowledge is only such as it is and can come to us only through those avenues of the knowing processes which, as human beings, we possess. It is, indeed, very likely that the world we know is not at all the same world which the centipede "knows," nor indeed the same world which a hypothetical God might "know." But to ask, Which of these worlds is the Real world? is wholly to miss the point and to misunderstand the nature of human knowledge. For such creatures as men are, the only world which is real is the world as it can be known by them through the only avenues of knowledge which they have, namely their senses and their reason. To raise any questions concerning the supposed nature of a Reality behind these is not merely to engage in speculation but to engage —certainly from the standpoint of "knowledge"—in idle and hopeless conjecture. For whatever one's conclusions might finally be, they could again only be arrived at through the only means at our disposal as human beings, since—most obviously—we have no other means.

The 'given,' then, is reality-as-it-confronts such creatures as men are *and* through such avenues of 'taking' as men possess.

The analysis of the 'taking' process is as much a natural part of psychological analysis as is any other aspect of any more than merely physiological human behavior. Such analysis, therefore, is another problem entirely and constitutes no part of the problem here under consideration.

To any possible—and, indeed, very likely—rejoinder that the here offered solution of the nature of the 'given' is altogether too simple, not to say: too obvious, I reply that there would seem to be no inherent reason why the 'given' should not be a simple matter. 'Given-ness' in every day life is certainly a very simple experience and its analysis has only been made unnecessarily difficult by the way in which philosophers have insisted upon a hopeless confusion of metaphysical and epistemological issues in their analysis of the problem. Once this confusion is avoided and the facts of 'given-ness' are dispassionately studied and analyzed, it appears to me that the solution of the problem follows almost as simply as it does immediately.

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Postulational Methods. I

BY

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INTRODUCTION

THE keynote of our investigation is suggested by a remark made by Enriques, "the young student of mathematics would indeed look in vain to the classical logic into which he was initiated for an adequate conception of the structure of a deductive science like Geometry let alone for an explanation of the value and meaning of the principles of such a discipline. What are axioms, postulates, definitions? What place do they occupy in the edifice of the theory? What criteria determine their choice and how can we establish whether they are acceptable or not?"[1] These are in general the questions we shall seek to answer in part and then we shall make a special analysis of two of the criteria determining the choice of the axioms.

Nowhere more than in modern methods of postulational technique does man appear so vividly as a creator. Man appears to create the world in his own images. He inserts a postulate and behold new elements are created. He inserts another postulate, as does Fraenkel in his set of axioms for the Mengenlehre, and behold elements are destroyed. He assumes certain postulates and deduces a number of theorems and behold he can control what he calls reality. What is this marvellous tool which does so much?

[1] The Historic Development of Logic, Frederigo Enriques, trans. by J. Rosenthal, New York, 1929, p. 3.

From the time of Aristotle to Bacon and Descartes the science of mathematics was looked to as the model for all the sciences to follow. Its methods were reputed to be applicable to all knowledge and to be the source of truth and discovery. Since of all branches of mathematics Geometry was the best developed, its methods were taken to be the method of all mathematics. Schoenflies says (Math. Ann 83) that Hilbert's foundation of Geometry (a purely axiomatic one) must be the model for all analogous investigations. The form in which Geometry appeared was what is now called *postulational.* Hence we have such an attempt as Spinoza's to use this method in the field of Ethics. That Ethics does need and since Socrates has needed a new method is an indisputable fact; and it is to Spinoza's credit that he attempted to use a new method; but there was no attempt to analyze the method of postulates to see whether or not it was applicable to such systems. For the most part, even today, the advocates of a postulational technique assume without question the validity of this method.

The reason for this faith in a postulational method is the fact that in such a method one finds exactitude of definition and rigor of deduction at their best. But it must be remembered that to use a set of axioms even in symbolic form does not necessarily imply the introduction of *metric properties.* If it did there would be great importance to the objection that such a method cannot apply say to Sociology. As will be evident later, all that axiomatic method does imply is that we set down in precise form the definitions we are using and the principles of deduction. In this way as little room as possible is left to objections based on misunderstanding of words or other ambiguities. The difficulty involved in precise definition cannot be taken as an excuse for vague terminologies, and is therefore not an argument against the use of an axiomatic. No one knows better than the mathematician how difficult it is to get precise and exact definitions. Yet the mathematician has advanced his science only by dint of continual redefining.

When we glance at the methods used by the disciples of the two sciences, in an effort to discover why Metaphysics is in-

volved in continual quibbles while mathematics marches triumphantly onward, we are struck as were so many thinkers, by the difference in the modes of attack.

If therefore we are to expect the other sciences to take advantage of so powerful a method, we must consider carefully the various aspects of postulate sets. How are the axioms obtained? What types of axioms do we have? What constitutes a definition? Is it proper to use the same type of deduction in all possible axiom sets? What is the general form of such a set? These are but a few of the questions needing a reply.

Of the various properties of postulate sets, *consistency* and *independence* have been considered the most important. Naturally enough an inconsistent set of postulates is valueless. Hence we must be able to decide when a set is consistent. This problem which today is attracting so much attention seems to have arisen during the attempt to *prove* the Parallel Postulate in Euclidean geometry. Since all direct efforts to prove this postulate failed, the attempt was made to deny it and to draw an absurd conclusion, as in the case of Saccheri's "Euclides Vindicatus (This method of proof is used by Euclid himself, cf. bk. IX prop. 12). Strange to say, no contradiction arose! This showed two things 1) the parallel postulate was independent of the other postulates 2) it was consistent with them as was its contrary. The method used was to show independence by consistency–a method which we shall see is still used despite its weaknesses.

Very little attention has been paid to the general analysis of postulate sets. The paper of Weiss "The Nature of Systems" is as close to this question as any one has yet come, but it is still not a very complete analysis. Further the paper of P. Hertz (to be cited later) defines certain concepts and draws a great number of theorems involved in general sets of postulates. For the most part it has been considered sufficient to say that an axiom set is composed of postulates, and theorems deduced from these and then to consider questions of consistency, categoricity, etc. merely in a general form. Or it has been said by psychologists, sociologists, etc., that such a method is foreign to the type

of matter with which they are dealing. Such men have in mind quantitative axiom sets and therefore see only one type of the axiomatic. Where methods of proof have been discussed, they have been discussed apart from axiom sets, as e.g. Mill's analysis. The question of the origin of axioms has been the most frequently discussed with points of view varying from attributing to them the status of an innate idea (as in Plato or Descartes) to a mere induction from observed events which can be overthrown by a more complete induction (as for Bacon or Mill).

In general one finds two schools of thought with respect to postulational technique. There are (1) the *formalistic* school of Hilbert Ackermann, and their disciples—represented in this country by Peirce, Sheffer and Huntington; (2) the *intuitionist* school of Brouwer, Weyl and their disciples. This latter school varies all the way from those who (like Brouwer etc.) accept the methods of postulational technique but direct their criticism to specific points to the Idealistic logician who denies the validity of these methods when applied to various disciplines.

It is therefore of importance that we analyze the theories of the two schools although we do not intend in this study to consider the Idealists arguments. Since for the most part theories within a given school differ only in detail, it will be sufficient to discuss the theories of Hilbert and Brouwer and to indicate as we proceed, any differences. This will give us a general idea of the methods in postulational technique, certain objections, an introduction to the question of independence and consistency and a foundation on which to base our analysis as postulational technique.

DAVID HILBERT'S THEORY OF PROOF

By 1887 *Georg Cantor* had invented the Mengenlehre. *Frege* had made an attempt to found Arithmetic on Logic. Everything seemed to promise great achievements. It seemed that the *Infinite* had at last been conquered. Then suddenly came the crash of the *Paradoxes*. How could it be possible that paradoxes could arise in mathematics? Certainly something was wrong!

It was necessary to save mathematics from skepticism! Could it be done? And now a strange thing took place. It was suggested that Mathematics in order to save itself, use the methods of mathematics—or to be more specific, of geometry.

In 1899 Hilbert had completed the axiomatisation of Euclidean Geometry and thereby established that science on a firm foundation. The idea that a science founded on axioms is certain and indisputable is not a new one, as has been pointed out, but goes far back. It is quite evident that Euclid's attempt to systematize geometry—which resulted in his Elements—was due to a feeling that there was an absolute science. This feeling was further evident in the efforts of Bolyai, Saccheri, and Lobatchevsky to prove the parallel postulate, since it seemed to be imperative that a certain science should be founded on as few axioms as possible, and these axioms should be as self-evident as possible. In Bacon also we find this emphasis on an axiomatic science. But the real impetus to axiomatic methods was due to Hilbert. In 1900, Hilbert insisted that despite the pedagogical value of genetic methods preference was to be given to axiomatic methods, because they assure a final validity and logical certainty. Again and again Hilbert insists that the only way to rid mathematics of its sore spots and achieve a safe foundation is to use the axiomatic method. It seems natural that Hilbert should rely so strongly on this method, since he has seen it give so safe a structure to geometry.

Hilbert however, perhaps because he was interested for the most part in mathematics, gave no thought to any analysis of the method itself. He took the method and used it. But he soon found himself faced with a problem which he had to solve or else acknowledge his method to be extremely precarious. This problem was that of *Consistency*. How do we know whether the axioms set up are consistent one with another? Certainly this question must be answered or else we are faced with the possibility of inconsistent axioms which may lead to absurdities. Hilbert himself saw the importance of this problem and tried to solve it. He succeeded in referring the problem back to the consistency of Arithmetic and finally to Logic, with the aid of his

students—Ackermann and P. Bernays. In doing this, Hilbert invented his new method which he called "Metamathematics." This method, which we shall discuss shortly, was an endeavor to put all transfinite methods of deduction into finite form and set all mathematics, as well as its proofs into a strictly formalistic shape. Fraenkel does not think that Hilbert's metamathematics is logic, but if it isn't it looks a great deal like Logic. The aim of Hilbert's work is to get a method by means of which all mathematics and its methods can be put into the form of formulas, and thus to be able to show that certain formulas cannot be demonstrated. In doing this, Hilbert draws a distinction, now evident, between a mathematical proof and a metamathematical one. A metamathematical proof is a proof concerning mathematical proofs, and it seems almost self-evident that such a proof must concern itself with Logic. That Hilbert felt his method to be closely related to Logic is evident from the fact that in 1928 there appeared a volume (written together with W. Ackermann) in which the foundations of his theory are expounded, and this book is called "Grundzüge der Theoretischen Logik." Metamathematics is to operate with the axioms, theorems, propositions as Arithmetic operates with numbers,—but that clearly is the way Symbolic Logic does operate with propositions (and axioms are propositions). In the introduction (p. 2) to this book it is remarked "In jüngster Zeit hat Hilbert den Logikkalkül dazu verwendet, um auf einem neuen Wege zu einem Aufbau der Mathematik zu gelangen, der die Widerspruchsfreiheit der zugrunde gelegten Annahmen erkennen lässt." ("Hilbert has recently used the logical calculus to attain a structure of mathematics by a new method which allows us to recognize the consistency of its assumptions.") We have perhaps overemphasized this point, but only because it is important to notice that Hilbert was forced to try to complete Logic before he could go ahead to a real consistency proof which he could not attain because of a defective logic.[2]

The problem is truly a very difficult one (as Hilbert himself

[2] Cf. Chwistek, *Ann. de la Soc. Polonaise de Math.*, 1923/25 who feels that Hilbert's metamathematics cannot be essentially different from the logical calculus although it is true that Hilbert believed his metamathematics was beyond Logic.

admits) and for that reason there have been some who claim it cannot be solved. But difficulty is not impossibility! (as Hilbert himself insists).

Metamathematics as Hilbert uses the term can be defined as *Mathematics in a purely symbolical form.* Its objects are the axioms, theorems and proofs of formal mathematics. Metamathematics generalizes mathematics until it makes evident the logical concepts and processes involved. It is, as Hilbert calls it a "Beweistheorie" which critizes the proofs of mathematics previsely as "the physicist investigates his apparatus." It is perhaps advisable to recall that Husserl defines the problem of Logic in a fashion almost identical with this.

It is a similar tendency to generalize which will lead us to postulational technique. The Theory of Postulate Sets attempts to get behind any given set and find what is common to all postulate sets. Clearly enough we easily reach the idea of Invariants for postulates sets. This will be developed later in another study.

In metamathematics we have formalized all mathematics. Clearly then, since there are logical ideas in mathematics, we must get symbols for the logical relations. These are the concepts: "implies," "and," "or," "not," "all x," "there exists an x." These concepts are symbolized as follows:

"implies" is denoted by $\rightarrow$ (logical implication, which Hilbert takes as defined[3]).
"and" " " " & (logical conjunction).
"or" " " " v (logical disjunction—material implication is meant[4]).
"not" " " " — (logical negation).
"all x" " " " (x) (apparently following Principia Mathematica).
"there exists an x" is denoted by (Ex).

Since now by means of symbolic logic all mathematics can be represented as a sequence of proofs in the form of equations, all

[3] But cf. Weiss' on the definitions of Implication which appeared in the Monist. Hilbert accepts the definition of Whitehead and Russell that $p \rightarrow q = p' + q$ which is a special case, as H. B. Smith has shown.

[4] Cf. Hilbert and Ackermann, *Grundzüge der Theoretische Logik.*

mathematics becomes a combination of such proofs (as e.g. in Peano's *Formulario*). A proof is defined as a figure of the following scheme:

$$\frac{\begin{matrix} S \\ S \rightarrow T \end{matrix}}{T}$$

where S and $S \rightarrow T$ are either axioms or theorems proven previously. Axioms for Hilbert, are formulas which *serve as* the foundations of the science. There is, of course, nothing said as to their nature, except that they be the *foundations*. Apparently on Hilbert's theory any theorem may serve as an axiom. We shall later note restrictions on this idea. It is sufficient to note that for Hilbert, these axioms serve for the deduction of other propositions either by substitution or by using the method of deduction, called a proof.

For the most part Hilbert is interested in the construction of a perfectly rigorous and consistent mathematics. And for this purpose he gives in various places axiom sets to define the number concept and the arithmetical operations. Clearly it seems to Hilbert that the method of postulates will bring about consistency. His whole effort is spent therefore in trying to get a real consistency proof—and that is the purpose of his "Beweistheorie." H. Weyl recognizes this fact when he says in a tone perhaps cynical, "The intention of the Hilbert proof-theory is to atone by an act performed once for all for the continual titanic offenses which mathematics and all mathematicians have committed and will still commit against mind; against the principle of evidence; and this act consists of gaining the insight that mathematics if it is not true is at least consistent."[5]

It is a question whether Hilbert is attempting to *atone* for these offences or *correct* them,—and also it is difficult to see what precisely this "principle of evidence" Weyl speaks of is. It would appear from other references and from Weyl's applications that this "principle" depends on certain intuitions which are undemon-

[5] Consistency in Mathematics, Rice Institute Pamphlets, *16*, 1929, p. 250.

strable. But we say with Dedekind, "Whatever can be proved, must not be believed without proof."

Since we are interested here in consistency proofs and not in the foundation of mathematics, we turn to Hilbert's attempts to prove consistency. As a good example of one method we take the proof given in 1922. Hilbert sets out to prove the consistency of the following set of axioms;—

1. $a = a$
2. $a = b.\rightarrow.a + 1 = b + 1$
3. $a + 1 = b + 1.\rightarrow.a = b$
4. $a = c:\rightarrow:(b = c.\rightarrow.a = b)$
5. $a + 1 \neq 1$

Now it must be kept in mind that Hilbert defines consistency as follows:[6]

A set of axioms is said to be consistent if when A is any demonstrable assertion, A^1 (the contradictory of A) cannot be proven from the axioms. Or in other words:—two contradictory assertions cannot follow from the same set of axioms if the axioms are to be consistent. As a special case then we have that: if a and b are "functionals" (i.e. variable propositions), then if in the given set $a = b$, then $a \neq b$ cannot be demonstrated. Or to take an illustration from arithmetic; If $5 = 5$ can be proven, then our axioms are consistent provided $5 \neq 5$ cannot be shown to follow. As is obvious at once, this really involves an impossibility proof with all its hidden dangers. E. A. Singer, Jr. once cited the example of a famous mathematician who "proved" that it would be impossible for a heavier than air machine to fly! It is rather a risky business to attempt to prove anything impossible,—and then suppose our proof fails, but does not show $a \neq b$ to be a consequence—i.e. suppose we can neither show that $a \neq b$ follows nor that it does not follow—then we have proven nothing. Any proof of an impossibility faces this objection.

But to return to Hilbert: He now proves the following lemmas

[6] Cf. the definition of consistency given by Wavre, in Revue de Metaphysique et de Morale, *31*, 1924, p. 460.

(the proof of which we omit but which are based on the above axioms):

Lemma I. A demonstrable assertion can contain the symbol $\rightarrow$ at most twice (i.e. on the basis of the above axioms).

Lemma II. An assertion $a = b$ is demonstrable only if a and b stand for the same symbols.

Having established these, Hilbert proceeds to the following

Consistency Proof. We are to prove consistency by showing that since $a = b$ is possible only if a and b are the same that $a \neq a$ is impossible.

Proof: To get such a formula we must substitute in axiom 5. But this says that

$$a + 1 \neq 1.$$

If then it is the result of a deduction, the second premise is of the form

$$S \rightarrow a \neq a$$

which must also be the result of a deduction since it cannot be obtained by substitution. We then have the second premise of this deduction

$$T \rightarrow (S \rightarrow a \neq a)$$

and from this we deduce in a similar fashion that

$$U \rightarrow (T \rightarrow [S \rightarrow a \neq a])$$

which is impossible.

Therefore our axioms are consistent.

At least Hilbert seems to think so. But putting aside any objections to the method of proof, it is obvious that Hilbert has here a circle. In the proof of the lemmas, which he uses to prove the consistency of the axioms Hilbert makes use of the very axioms whose consistency is in question. He thus assumes that the axioms are consistent and proves the lemmas, and then uses the lemmas to prove the axioms consistent. It is significant that Hilbert does not use this method of proof again, perhaps because he recognizes its circularity. It is surprising that Becker should say, referring to the above proof: "Das angeführte Beispiel zeigt wohl zur Genüge, dass derartige Unmöglichkeitsbeweise unter gewissen Umständen möglich sind."[7] ("The illustration given

[7] Cf. Becker, Mathematische Existenz, Jahrbuch für Phil. VIII, Halle, 1927, p. 457.

indeed shows to satisfaction that such impossibility proofs are possible under certain conditions.")

It will be noticed that the axioms given above are finite, i.e. they apply only to a finite number of elements. Hence it is necessary to introduce axioms for the infinite. This, as Hilbert points out in complete agreement with Brouwer, cannot be done except with great care, because for infinitely many objects the assertion "it is false that all these objects have the property A" has no *precise* meaning, since the contradictory of "all objects *a* have the property A" is "there exists an object *a* which does not have the property A." Since this latter can be shown only by considering an infinite quantity of objects, it cannot be shown at all.[8] So far Hilbert and Brouwer seem to agree, but they differ in their solution to this problem. Brouwer rejects the usual form of the law of excluded middle for infinite classes, while Hilbert says "if we cannot show such an element, we might be able to come to contradiction on the assumption that there does not exist such an element and hence prove our proposition by a reductio." This method of proof is used to a great extent in Point-Set theory in mathematics, and a failure to make careful distinctions would cause one to decry the consequences of accepting Brouwer's view.

Since Hilbert wishes to preserve arguments concerning infinite sets, and since he feels that he can give meaning to such arguments, it is necessary solely to show the *consistency* of his transfinite axiom with the other axioms. This being done, no contradiction can arise. Brouwer's objection would be, or course, that even if this is so, the fact that we cannot *construct* an element verifying our statement shows that we have made no significant advances. Consistency alone is not enough—constructability also is necessary.

Since we wish to show the second type of consistency proof that Hilbert gives, we introduce this transfinite axiom and proceed to explain how its consistency with the other axioms is demonstrated.

[8] Cf. Hilbert, Mathematische Annalen., 88, p. 154–155.

Axiom:

$$A(\tau A) \rightarrow A(a)$$

which reads "If τA is an individual which represents all a and we can predicate A of τA, then we can predicate A of all a."

For example: if τA is a triangle and we can predicate of this triangle that its angle sum is 360 degrees, then we can predicate of *all* triangles that their angle sum is 360 degrees.[9]

The proof of the consistency of these axioms now proceeds as follows: A process is defined whereby the formulas of a given proof whose final form is numerical are all changed into numerical equations, and we show that by this method we obtain a system of true formulas. In other words, we have a method of specialization followed by exhibition of a consistent set of numerical equations so that we can never end with an inconsistent equation. Notice that this reduces the process to a finite one.

We then assume we have a proof which leads to the absurdity $0 \neq 0$. We wish to show that such a proof is not possible.

In our axiom $A(\tau A) \rightarrow A(a)$, we specialize $A(a)$ so that it means the equation

$$f(a) = 0,$$

where $f(a)$ is an integral function. Then

$$\tau(f) = \tau_a (f[a] = 0)$$

defines a whole number correlated with $f(a)$ and our axiom becomes

$$f[\tau(f)] = 0 \rightarrow f(a) = 0.$$

This is satisfied if we define $f(a)$ and $\tau(f)$ as

(1) $\tau(f) = 0$, when $f(a) = 0$ for all a.

(2) $\tau(f)$ is the least number such that $f(a) \neq 0$ whenever $f(a) \neq 0$

We now specialize f and assign it a definite value φ, then $\tau\varphi = 0$. Then

$$\varphi(0) = 0 \rightarrow \varphi(z) = 0$$

where $\varphi(z)$ is constant whenever z is. Now, is $\varphi(z) = 0$ for all z or not?

[9] The law of excluded middle is proven consistent with all these axioms in a similar fashion by Ackermann, in Mathematische Annalen, *93*, pp. 1ff.

If it is, then we can never get $0 \neq 0$. Suppose then it is not, and let $\mathcal{T}(\varphi) = z$, then

$$\varphi(z) = 0 \rightarrow \varphi(S) = 0,$$

Since by assumption $\varphi(z) \neq 0$, this implication is a valid one, since in that case $\varphi(z) = 0$ is false and implies any proposition. In every case —whether or not $\varphi(z) = 0$—we can get a special case in which the transfinite axiom is valid and therefore $0 \neq 0$ can never occur.

Thus Hilbert proves the consistency of the transfinite axioms. This method is not exactly a direct search for an interpretation, but secures this by working backward replacing variables by constants. It is rather an attempt to show that all implications involved are valid ones, and hence cannot lead to a contradiction. There are certain assumptions involved which, to say the least, weaken the strength of this proof. They are:

(1) The proof that $0 \neq 0$ which is assumed to exist, takes place in a finite number of steps.
(2) Arithmetic is a consistent system.
(3) It is always possible to find constants with which to replace the variables such that we can show $0 \neq 0$ *always* impossible.

If (1) is not true, our proof that $0 \neq 0$ cannot occur, becomes impossible.

If (2) is not true, then we cannot argue on the basis of Arithmetic.

If (3) is not true, we have not been able to find interpretations for our variables and we have proven nothing. Furthermore, it is quite possible that on one interpretation we get a contradiction while on another we do not. E.g. $x \cdot x = x$, will if interpreted in arithmetic lead to the absurdity that $4 = 2$, but in logic we get no inconsistency.

As a consequence of these remarks, we must admit that Hilbert's methods of proving consistency are inadequate since they work only for special cases.

In conclusion we may say that Hilbert properly insists that all assumptions be made explicit and this can best be done by the axiomatic method. Since we are stating axioms, we must be

sure that our axioms are consistent with one another, else we could prove anything whatever; therefore a consistency proof is essential. However, Hilbert's methods are not adequate as general proofs of consistency. Precisely what Hilbert means by metamathematics is vague, but seems to be very much like logic. This seems to indicate a tendency to push the problem of consistency into logic; but a logic which takes no account of modality.

L. E. J. BROUWER AND INTUITIONISM

In a lecture held in September 1923, A. Fraenkel pointed out "*Dass die Anwendung des Tertium non datur stets ohne Gefahr ist bestreitet der Intuitionismus überhaupt nicht, sondern nur, dass sie berechtigt ist.*" ("Intuitionism does not contest the fact that the application of the law of excluded middle is always without danger, but that it is justified.") If I understand what Fraenkel meant, it is I feel, one of the very few remarks correctly interpreting Brouwer's point of view. But for the most part, the suggestion thrown out by the remark seems to have been quite overlooked. Brouwer was, and still is, misinterpreted on the basis of one or two of his papers. Since Dresden translated "Intuitionism and Formalism" *philosophers* have scorned Brouwer with the remark that he denies the law of excluded middle, and *mathematicians* have refused to listen on the grounds that accepting Brouwer's doctrines meant the destruction of a great part of mathematics. It will be seen later that both these views are incorrect. It is not very surprising that philosophers should be unable to understand Brouwer because of the technical mathematics involved, but it is strange that the mathematician has given so little attention to the warnings.

Many expositions and criticisms of Brouwer's doctrines have been written based on a study of a few papers, with a consequent misinterpretation which Brouwer himself repudiated. When we consider Brouwer from the point of view of his entire development and all available writings, including the papers of Heyting which have met with the approval of Brouwer, it becomes evident that Brouwer is insisting upon a very important factor in mathematical reasoning, and is pointing towards the application of a more

general logic to transfinite mathematics. To the method advocated by Hilbert—which is clearly Rationalistic or Idealistic there is the reaction unfortunately named Intuitionism, but which is much more akin to Empiricism.[10] At any rate, Brouwer's insistence on "constructability" is more analagous to Empirical doctrines than to Intuitionism. Both schools start from certain intuitions and neither can, as a result, claim the title.[11] We shall see that there are similarities between the two, and that they are advocating doctrines which, in a sense, are complementary.

The fact that Brouwer had repudiated all expositions of his thought makes it extremely difficult to determine precisely what he maintains. We can but analyze his writings and risk our interpretation being repudiated also.

Since we are concerned at present only with postulate sets and Independence and Consistency we will consider only those of Brouwer's views which concern our problem.

Hilbert endeavored to set up a strict axiomatic, and for him the question of consistency becomes of paramount importance. Brouwer, on the other hand, refuses to accept such a foundation as entirely sufficient. The axiomatic method gives only a necessary condition of truth, not a sufficient one. How do we know that the axioms we have set down can be applied to existing entities? How can we prove consistency? We have already shown that Hilbert's consistency proofs are not of the best, except in so far as they do *construct* a system satisfying the axioms. Lebesgue had already emphasized the point that mathematical existence can be shown only by actual construction. And Brouwer insists very strongly upon the same idea.

When we have set down as an axiom the law of excluded middle, how, asks Brouwer, do we know it can be universally applied? True, in the case of finite sets of objects, the law does hold, since in a finite number of steps we can either prove or disprove it—

[10] Brouwer himself accepted the names Intuitionism and Formalism. Cf. Bull. American Math. Soc., 1913.

[11] But cf. Wavre, "Y a-t-il une crise des Mathematiques?" Rev. de Metaph. et de Morale, v. 31, p. 435.

but what about infinite sets of objects?[12] Hilbert's reply would be that since the *Tertium* is consistent with the other axioms of logic, it does apply here. But notice that Hilbert is perfectly aware of the danger in transferring this law to infinite sets before *proving its consistency*. For Hilbert once consistency is shown, the question is solved—and Ackermann has demonstrated this consistency. For Brouwer, however, the question is not so easily disposed of. Take an example:

Suppose we have a set of objects (S) and we say: Either all objects (S) have property A or not. In order to show that (S) objects do not all have the property A, we must define what we mean by the statement when there are an *infinite* quantity of elements in (S). Clearly if A is the defining property of (S) [i.e. the property by means of which we characterize (S)], then there exists no element in (S) not having the property A—by definition. If A is not the defining property, then the obvious meaning of our proposition is: "There exists an element in (S) which does not have the property A."

The way then to show that "not every element has the property A" is to show that *at least one element of* (S) *exists* which does not have this property. The method of showing existence is only by an *actual construction* or *pointing out*. In an infinite set of objects we may never be able to *construct* such an element. As a consequence we cannot significantly apply the law of excluded middle to infinite sets,—at least not under the interpretation of *Truth = Existence* as defined above.

It has usually been assumed that Brouwer has therefore denied the Tertium completely and hence such a proposition as "A proposition is either true or false" becomes meaningless for him. We feel that Brouwer has been misinterpreted and in this respect he is correct and Hilbert wrong. We point to the fact that Hilbert follows *Principia Mathematica* very closely and as a result maintains a two-valued logic, i.e. all the propositions are either true or false. But Brouwer is consciously or unconsciously

[12] Brouwer essentially makes use of the law in proving that for *Finite* sets, a proper subset cannot be of the same cardinal number as the finite set itself. He assumes that a proper subset of a finite set is either of the same cardinal number as the set or it is not. To assume that it is leads to a contradiction; therefore it is not. Cf. *Begründung der Mengenlehre*, etc. Amsterdam 1918, p. 6.

pointing to what has developed into a logic of modalities (possible, etc.).[13]

The law of excluded middle is as stated by Coffey, "One of any pair of contradictories must be true; both cannot be false together" (Science of Logic, London, 1912 p. 24–5). When applied to propositions it has been assumed that the contradictory of *True* was *False*, and that falsity was equivalent to *not-true*. But the logic of modalities has shown that this is not so. A proposition is either true, or possible, or impossible, or false etc. A complete logic of truth values still remains to be developed. As a result to say that a proposition is not true is to have excluded one meaning of "truth" but *still leaves a range of possibilities open*. Perhaps that is the real basis of Brouwer's objections to the use of the tertium as heretofore made.

Consistency alone then, for Brouwer is not sufficient. For actual validity a method of construction must be given. Or we might say that for Brouwer *consistency as validity appears to be constructability*, or else we have merely some degree of truth and not truth itself. From which it would obviously follow that since no general method for constructability can be described, since different objects are constructed in different fashions, then no consistency proof is possible. The problem is for Brouwer changed to a problem of "existence." But it must be kept in mind that constructability alone does not constitute all of Brouwer's contribution. It is evident that no infinite set—law abiding or not—can *actually* be constructed. All that we can do is to define a law for any element. It becomes evident that this term is a very vague one and would not amount to much in itself —at least not until more closely defined. Brouwer's real contribution lies in pointing to the need of a new logic (of modalities) in the sphere of transfinite mathematics. All attempts to reduce Brouwer either to denial of the tertium or an insistence on con-

[13] It is true that in "Intuitionism and Formalism" and in "Intuitionistische Mengenlehre" Brouwer explicitly rejects the law of excluded middle. But that he does not mean to reject it "in toto" is evident from the statement that the law does hold for finite spheres. Certainly from the article in Sitzungsberichte d. Preuss. Akad. Wiss., 1928, one might hastily conclude that Brouwer intends to reject the Tertium.

structability entirely miss the real issue. From the real issue there comes the need for a re-interpretation of the law of excluded middle, and as a special corollary the need for a special type of constructability.

We might sum up the debate as follows: For Hilbert the possible as well as the actual is the True, and the impossible is the False. So that if we show that a given theorem is impossible, we have demonstrated its falsity, and conversely. And also if a proposition is impossibly impossible, it is true. If we denote "p is possible" (where p indicates *any* proposition) by

$$|p|$$

and the proposition "p is impossible" by;

$$|p|'$$

we can then symbolise the disputed principle as follows;

$$||p|'|' = p$$

Where p by itself mean that p is true. The acceptance of this principle leads to the destruction of a great many important distinctions. For Brouwer this would read

$$p \rightarrow ||p|'|'$$

(This is the so-called Brouwer axiom.)[14]

We again emphasize the fact that Brouwer agrees with Hilbert in the sphere of the finite. But of course, in the sphere of finite mathematics, propositions can be shown true or false by construction, and therefore a logic based only on these two truth-values is applicable. Even here as we shall see, reservations must be made, since it is not true that we can therefore construct every proposition.

In non-finite mathematics, however, the case is other—a theorem may be possible without being actual. Hence we are compelled to introduce a logic of modalities—a fact Brouwer felt. Brouwer explicitly points out in an article in *Journal für reine und*

[14] Cf. Wavre—*Logique Formelle et Logique Empriste*, in Rev. de Metaph. et de Morale, 1926, p. 66 and p. 69ff.

angewandte Mathematik (vol. 154, p. 1f) that in finite mathematics the law is valid, but elsewhere it can lead to false theorems. He then cites various theorems as;

1. For any two different points of the continuum either $a > b$ or $b > a$.
2. Every continuous function $f(x)$ defined over a closed interval has a maximum.

and gives actual illustrations showing where they do not hold. So far as I know, no mathematician has taken the trouble to point out where Brouwer's illustrations are invalid—*if they are.*

Let us apply this interpretation of Brouwer to the famous Fermat problem "Do there exist three integers x, y, z such that $x^5 + y^5 = z^5$?" Now the mathematician would argue as follows: Suppose there did not exist such numbers, then what must follow? If he could on this assumption show that an absurdity resulted, he would conclude that such numbers did exist *even though he had not found them.* Brouwer would not accept the final conclusion. He would maintain not that such numbers actually do exist even though we do not know them, but that only the *possibility* of their existence has been established. In other words, the law of contradiction and interchange is not to read:—If " 'q is impossible' implies 'p is impossible' " then " 'p is true' implies 'q is true' "—but, If " 'q is impossible' implies 'p is impossible' " then " 'p is possible' implies 'q is impossible,' " which when verified for p gives " 'p is actual' implies 'q is possible.' "

The truth of the Fermat problem for $n = 5$ is shown only when the numbers x, y, z are constructed. In other words, an existence proof must be a method of construction.

Another interesting application of this mode of reasoning is the solution of a well-known antinomy.

Let (M) be the class of all classes not containing themselves as members. Then is (M) a member of itself or not? Suppose (M) is a member of itself, then it is a class which does *not* contain itself as member—by definition. Therefore, since our supposition has led to an absurdity, (M) is not a member of itself. But if it is not a member of itself, it

satisfies the definition of the class (M) and therefore it is a member of itself. We are then led to a paradox.

But notice that from Brouwer's point of view, when we show that the assumption (M) is a member of itself leads to a contradiction, all we can deduce is that it is *possible* that (M) is not a member of itself—and it is possible that (M) is a member of itself. Which is no longer paradoxical, since if it is *possible* that x is y, it is also *possible* that x is not y. The theory of types which states that the question is meaningless is therefore not needed. It is evident that under the conditions given both cases are possible and will always remain so.[15]

Such a view does not invalidate a great deal of mathematics, as has usually been supposed, but merely makes theorems using this principle, not certain but only *probable*. From the point of view of a pure theory, it is possible to assume that

$$\|p\|'|' = p$$

since Dr. Parry in Lewis and Langford's text has shown Brouwer's axiom to be consistent, and we offer a proof in a later issue that the converse is also.[16] Brouwer's argument does not of itself invalidate as much of mathematics as it at first glance appears to do.[17] His own attempts to set up the same theorems on new foundations show that Brouwer does not think so. It appears quite possible that a suitable definition of (say) infinite sets and infinite numbers of choices will allow us to retain all mathematics. To say that the means used to arrive at conclusions are wrong, does not imply that the conclusions are incorrect. In other

[15] In this connection it is interesting to quote from the *De Interpretatione* of Aristotle—"From the proposition *it may be*, it follows that *it is contingent*, and the relation is reciprocal." Cf. 22a, 14–15.

[16] Such an assumption is not inconsistent with the other logical assumptions and hence no inconsistent results are reached; however a great many important distinctions are wiped out, and it is these which Brouwer is insisting upon. Fraenkel's *ad hominem* argument in Erkenntnis 1, 1930, p. 292, is therefore no argument at all. (Die Heutigen Gegensätze in der Grundlegung der Mathematik.)

[17] Brouwer emphasizes again and again that now we can construct mathematics on a rigorous foundation. Heyting further sets up a new Projective Geometry on Intuitionistic foundations. Cf. Sitz. Preuss. Akad. Wiss., 1928, p. 51 for Brouwer's remarks and Math. Ann., *98*, for those of Heyting.

words, to deny a premise or premises (i.e. to deny that $\| p |'|' = p$) and to infer therefrom that the conclusion is false, is to commit the fallacy of denying the antecedent. And this fallacy has been committed by many with respect to Brouwer's position. Brouwer has maintained that many theorems of the "Theory of Aggregates" have been deduced by illegitimate methods—therefore (sic.) the claim that Brouwer denies that these theorems are true. One need not, as a result, be fearful (as is e.g. Dubislav) of the catastrophe which befalls mathematics.

It appears that Brouwer, in constructing his Mengenlehre, makes use of the concept "Gesetz." An aggregate for him is not a collection of objects as Cantor defined it, or "eine Vielheit, as Einheit gedacht" as for Hausdorf, but rather a "Gesetz"—a law—by means of which whenever any object is selected, any such object either does or does not define a definite symbol, or else involves the definite rejection of the result. In other words, an aggregate is a function, the evaluation of which shows which objects can be called "elements of the aggregate"—where the "element of the aggregate is defined as those objects which satisfy the function." It appears clear that a great many of the paradoxes are due to ill-defined concepts. And again Fraenkel's attempt to eliminate the paradoxical aggregates by the addition of a new axiom is an attempt to delimit more closely the terms involved. It would appear to be more profitable and more elegant to redefine "Menge," rather than to add axioms.

This method of attack appears to substantiate our interpretation of Brouwer's criticism of the law of excluded middle. For obviously, to determine whether or not a given set of objects have a property E we need only substitute in the function and "compute" the result. The result will then indicate whether any given object does or does not have the property or whether the property is meaningless with respect to that particular function. In his article "Intuitionistische Mengenlehre," Brouwer makes a comparison of his results with the formalist Mengenlehre. From this we gather two important results:

(a) By use of his doctrines all paradoxical aggregates are avoided. This should, for any mathematician, be a cardinal

virtue of any doctrine, and at least compel a close consideration of the ideas involved.

(b) The difference between the theorems in Brouwerian Mengenlehre and Cantorian Mengenlehre is not so much one of contradiction as one of generality. Where Brouwer rejects a theorem, it is because under the condition of *Constructability* that theorem cannot be demonstrated. It is important to remark, that from Brouwer's own doctrine, that does not *invalidate* the theorem, but merely does not prove it *true*. The argument comes down to the legitimacy or non-legitimacy of

$$p = \| p |'|'$$

Accepting this and Hilbert's theorems are valid, *even though essential distinctions may be lost.*

In all this it is evident that Brouwer does not attempt to construct an axiom set. His entire method is characterised by that fact. Brouwer defines terms and proves theorems analytically. But it is evident that he uses a great many principles which he at no point states. The enunciation and explicit statement of these principles can form a set of axioms. But on the basis of Brouwer's fundamental intuition, on which all mathematics and logic is founded, we can, as Dresden points out, set up a number of different axiom sets, despite the fact that von Neumann believes the essence of Intuitionism contradicts the axiomatic method. This I do not believe to be so, because the very essence of an axiomatic is to make explicit all tacit assumptions, and to uncover any ambiguities and limitations. One axiom set based on Brouwer's ideas is given by Heyting and another by Becker. We shall have more to say of these later when we discuss a logic of modality.

In an article in 1928[18] Brouwer points out what he considers to be the four points on which he and Formalism are at odds. He feels that his views have been, and are penetrating all mathematics despite the violent denials of Formalists. These points are:

[18] Sitz. der Preuss. Akad. der Wiss. Phys.-Math. Klasse, 1928, p. 48ff.

1. The distinction between the efforts to construct the whole of mathematics as a body of formulas and an intuitive theory of the laws of this construction, as well as the recognition that for the latter theory the set of natural numbers is indispensable.

This clearly indicates that Brouwer does not deny the possibility of axiom sets for mathematics, but is interested in the laws involved in developing mathematics from such a set. No matter how intuitive the theory, it can be expressed as a postulate set. The demand for the natural numbers as an intuition need not concern us here, since we are not discussing the foundations of mathematics.

2. The rejection of the uncritical application of the logical law of excluded middle, as well as the recognition *a*) that the investigation of the reason for the validity and sphere of applicability of the law is an important object of mathematical foundations; *b*) that this sphere of applicability embraces in intuitive mathematics only finite systems.

This clearly shows that Brouwer has recognized the lack of generality of the law of excluded middle. The emphasis on constructability is due to the fact that when a system can be constructed, then the less general law of excluded middle does apply. Existence has been equated to constructability for reasons relating to the Tertium. Unless an entity can actually be constructed, we have the mere possibility of the entity, and all our arguments relating to this entity are *possible*, since they are contingent upon a possibility. And consistency is in a similar condition. To prove consistency for a set of postulates is to do something, but not all that is necessary. As will be shown later, we have a set of postulates consistent with a given axiom, and also with the converse of the axiom. True, we have then alternative systems, but we must also recognize a further property of a set of propositions of which consistency is a special case—i.e. the property of generality or sufficiency.

It is also clear that on Brouwer's view proving consistency by means of illustrations is only a special type of consistency proof.

This demonstrates further that Brouwer, although perhaps not

consciously, was pointing to a logic of modalities. The fact of Heyting's set and Becker's set clearly demonstrate that at least the logic of modality follows as a consequence from Brouwer's ideas.

3. The identification of the law of excluded middle, with the principle of the solvability of every mathematical problem.

It has been argued that since every proposition is either true or false, every mathematical proposition can be proved or disproved. In other words, the application of the law of excluded middle to mathematics leads to the belief in the solvability of every mathematical problem. Brouwer, of course, rejects this faith. If solvability means that it can be shown to be either true or false, then, on Brouwer's arguments, this need not be possible, as in the case of the paradoxical aggregate referred to before. Apart from Brouwer's arguments, it does not seem to be a legitimate deduction to say that because a proposition is either true or false, it can be *shown* to be true or shown to be false. Yet this has been an article of faith on the part of the mathematician until recently.

It is evident that the recognition of the views of Brouwer will make the attack on the "Entscheidungs-problem" vastly different.

4. The recognition that the justification of Formalistic mathematics by the proof of its consistency contains a vicious circle since this is based on the validity of the proposition that the consistency of a proposition implies its truth.

This is a recognition of the fact that until consistency is defined in logic, all proofs of consistency must contain a *petitio*. To argue that we can show consistency by giving a practical application is to assume the principle that "a consistent proposition implies its truth" is consistent with the logical principles, and the Hegelian principle that the real is the consistent. We shall later have more to say concerning the assumption involved in such a consistency proof.

Certainly it appears reasonable to expect logic to be the definition of consistency. Lewis's definition of pOp is an entirely different concept from the one we are dealing with. We leave this also for a later chapter.

In conclusion, it is remarked that the two schools are not contradictory fundamentally, but rather different with respect to the demand for rigor and generality.

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The discussion of postulate sets will be continued in later issues of this journal. We shall in the next article begin to dissect the Set.

Methodology of Modern Physics

BY

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V. ONTOLOGICAL STATUS OF THE CONSTRUCTS OF EXPLANATION

DO MASSES, electrons, atoms, magnetic field strengths, etc., *exist*? Nothing is more surprising indeed than the fact that in these days of minute quantitative analysis, of relativistic thought, most of us still expect an answer to this question in terms of yes or no. The physicist frowns upon questions of the sort: is this object green?; or what time is it on a distant star? For he knows that there are many different shades of green, and that the time depends on the state of motion of the star. Almost every term that has come under scientific scrutiny has lost its initially absolute significance and acquired a range of meaning of which even the boundaries are often variable. Apparently the word *to be* has escaped this process.

A glance at the multifarious usage of the word exist is sufficient to convince anyone of the confusion which "exists" in connection with this point. The chair exists because I can see it; the United States exist; a definite integral exists; fear exists in the minds of people; fairies exist in my imagination. I have been using the word, I think, in a manner sanctioned by refined discourse in all these instances. Clearly, if there is no more uniformity in the standard usage of the word we had better drop the question as to the existence of the constructs of explanation.

But perhaps there is a way out. We may classify a suitable number of modes of existence and then state in which of these modes electrons and so forth exist. Without claiming more than a provisional or illustrative character for this particular classification I propose to use the five preceding examples as typifying five modes of existence. It is then much simpler to answer our question, but even so we can expect no uniform answer for all cases. Masses as well as the quantities time and space, which figure prominently among constructs, exist in a sense much like that in the first example. Electrons, atoms, etc., certainly belong to a different class for they can not, in principle, be perceived. Some may perhaps be willing to ascribe to them a mode of existence such as is possessed by the United States, others may feel, rightly, that our classification is not complete and that we should provide a separate compartment in our scheme for entities of this kind. Some of the abstract constructs employed in quantum mechanics (Cf. § VII) exist in the third mode of our classification, and there are no constructs that exist in the fourth or the fifth. Our scheme is seen to become highly artificial, but why should it not be artificial? Physical science has not developed along the lines of any of the traditional meanings of "existence," and it is perfectly natural that its constructs, when analyzed according to an extraneous principle of division, should present a bizarre appearance. The main point we are making is this: physical constructs can not be said either to exist or not to exist; their ontological status has to be fixed in accordance with a more elaborate analysis of the meaning of existence. In particular, the value of a construct bears absolutely no relation to its mode of existence.

It is apparent that the term "universe" must offer similar ambiguities. We are certainly at liberty to standardize the term in any way we please. A possible choice would be to combine all modes of existence into a universe of all things. Many prefer to include everything that exists in the manner of the chair into the universe and exclude everything else, a procedure which the critical physicist is unlikely to adopt. Logicians feel free to distinguish several universes of discourse, each containing a specifiable

class of things. We are not particularly interested in any special one of these definitions, but rather in the patent fact of the freedom which we can exercise. We shall define the *physical universe* as the class of all data (nature) and of all constructs of explanation. This, we feel, is most strictly in accord with the judicious use of the term.

This whole story could be repeated in connection with *reality*, but we shall not belabor the point ad nauseam. Mention might be made, however, of an etymological consideration which could serve to regulate the usage of this word. The origin of real, the Latin res, is just as diffuse as our word thing and will not help us in setting up a standard of usage. Let us recall, therefore, that real is the very exact translation (in point of meaning) of the German *wirklich*. The origin of this word is beautifully clear: *wirklich* is anything capable of acting upon, or influencing, anything else. If we use real in this sense the word loses its force, for practically anything would then be real inasmuch as it can influence our minds; but it also loses its menacing stare, and people will no longer be mystified by a reference to reality. The world would be happier if this were true! For my part, I shall use the word in this way; or rather, I shall refrain from using it, observing that it is so weak.

VI. CLASSIFICATION OF CONSTRUCTS

There is a point of view from which constructs can be classified more naturally than was attempted in the preceding section, a point of view that will at the same time afford interesting glimpses of the historical development of physics.

Let us consider first the field of mechanics. Here we find a great number of notions which appear to have their origin among the data of nature. On closer examination it is seen, however, that they are not truly identical with data but are derived from them by simple processes of abstraction. All large scale mechanical models are of this type. Such constructs are capable of being visualized, they are "anschaulich." By virtue of their suggestiveness they recommended themselves most directly for use in an explanatory symbolism, and it is by no means surprising

that they played such an important rôle in all early theories. It is important to recognize, however, that no theory can consist of visualizable constructs alone; the models must be furnished with properties which enter the stage as definite quantities like fields of force, potential energy, etc., and these are in general not visualizable. We shall indicate the close relation which this important group of constructs bears to nature by calling them *sensible* constructs, sensible not in its appreciative but in its denotative meaning.

The next and probably more important group contains electrons, atoms and many similar entities. It is not uncommon to find people believing that these are sensible in the same manner as the former class. To them, an electron is a very small charged sphere, precisely like an insulated metal shell that has been touched by a charged body. And an atom is simply a lot of such little charged spheres moving about a more concentrated group of smaller but more massive spheres, called a nucleus. But they forget that in the process of making the spheres as small as they need be, the very attribute of sensibility is lost. There is no way in which a sphere smaller than a wave length of light can be imagined, just as there is no way of assigning to it a color. The claim that an electron be an "anschauliches" model is quite as anachronistic as a revival of Anaxagoras' thesis that atoms (homeomerics) differ by their tastes and colors. Moreover, every imaginable charged sphere possesses a mass besides that which is associated with its net charge, an electron does not. For similar reasons atoms, protons, neutrons etc., in fact most elementary physical systems, are not sensible. We shall not deceive ourselves by believing them at all imaginable, and therefore call them *pseudo-sensible* constructs. They borrow, it is true, a number of aspects from the realm of nature, but possess other properties which permit us to say that they could never be the object of sensory experience.

Of course there are experiences which can inform us, by a series of plausible inferences, of the presence of an electron. This series of influences amounts to the correspondence between constructs and data of which we have spoken. Replacement of

correspondence by identity in this connection would, I believe, be a logical error. Nor should we say, except metaphorically or for the sake of brevity, that the electron causes a track in a cloud chamber, a point which we have also discussed previously. It is well to emphasize that this formal attitude, so distasteful to many physicists, by no means renders the vivid association between their little particle and its visible path illusory; for there is no reason why a more abstract relation can not be as impressive and vivid as one that is concrete. The great advantage of the attitude is, beside its logical consistency, the ease with which it allows the transition from one pseudo-sensible construct (e.g. little charged sphere) to another (electron wave) when such a transition becomes necessary.

Most quantities, i.e. constructs susceptible of epistemic definition, are pseudo-sensible. Consider, for example, the notion of kinetic energy. It involves somewhat vaguely the conception of a moving body, in particular an abstraction so typically expressed as *vis viva*. But this sensible characterization alone would not be sufficient to distinguish it from momentum, hence we take refuge to mathematical description and define the one as $\frac{1}{2}\, mv^2$, the other as mv. Temperature suggests features of the sensation of hotness, yet there is no way of defining physical temperature adequately by reference to this sensation. Finally, let us take wave length. When the term is used in mechanics it represents a sensible construct, since the length of a water wave can be the immediate object of perception. In acoustics, however, the character of the term undergoes a distinct metamorphosis, and it emerges as a clearly pseudo-sensible construct in optics.

Historically this latter class of physical notions emerged nearly simultaneously with the sensible constructs, but the two types were largely confused, not so much because of carelessness in the analysis of the concepts as because of the imperfection of the early theories themselves. The fact that atoms are imperceptible, for instance, could not be known before the wave theory of light was developed. It is very essential at present, however, that we recognize the abundance, even in the more primitive systems of physical thought, of elements which make no direct appeal to

nature, for it is these elements which distinguish the most recent theories. Pseudo-sensible constructs form a link between the former sensible type and the abstract ones which are now to be considered.

We have seen that an electron according to the conventional prequantum view is, crudely speaking, a piece of nature with its properties modified in an unnatural manner; it is indeed a peculiar mixture of sensible and abstract attributes. As far as its sensible portion is concerned one can not be quite sure whether it is a particle or a wave. But it is impossible to maintain that it is sometimes one and sometimes the other, since that assumption would violate most flagrantly the first of the requirements laid down in section IV.[13] Thus arises the question: Is it not possible, by suppressing altogether the sensible aspects of the construct, to arrive at a notion which is free from all conflict? To clear the way for an affirmative answer we first observe that our efforts of analyzing physical thought have exposed no requirement which would bar totally abstract constructs from admission into physical theories. To be sure, there are psychological reasons why they would be considered last, but apparently the time is ripe for their consideration. Ask anyone who presses the use of physical models to give the reason for his insistence; if he faces the issue squarely he will either say that all good theories employ models, which, of course, is not strictly true and besides irrelevant, or he will point to the difficulty and complexity of abstract theories, which simply means that he is more conversant with the others and perhaps unwilling

[13] Occasionally one finds in the literature the assertion that physicists apply the wave theory for certain purposes of calculation, the particle theory for others, and that they must know in advance which theory to apply in order to get the correct result. If this is taken to reflect the physicist's basic attitude, then the statement is grossly misleading. The point is that we have a reasonably satisfactory theory of the electron which assigns to it neither the character of a particle nor that of a wave. In principle, this theory can be applied to all specific cases; but its rigorous application is often very cumbersome. Fortunately we know, however, that in certain limiting cases this very theory leads to the same results as the wave theory, and in others it leads to the results of the particle theory. On the basis of this circumstance we often permit ourselves to use either of these latter, simpler theories in the solution of specific problems, or even to illuminate our discourse by reference to particles or waves. But there is absolutely no conflict between these notions.

to learn anew. We must admit the legitimacy of constructs wholly insensible, constructs which permit no more visualization than a mathematical formula.

Quantum mechanics actually replaces the electron, and any other physical system, by an *abstract* construct of this sort. Previously it was presumably round, of definite size and mass, and it carried a certain charge. Now it is described by certain mathematical symbols called Hamiltonian, momentum operator and others. These symbols are placed in correspondence with nature, much in the same way as the previous sensible and pseudo-sensible constructs, and the resulting system of explanation is just as satisfactory and proper in a critical sense as any other. The question as to the "meaning" of Hamiltonian and momentum operators is no more embarrassing than that as to the "meaning" of imperceptibly small particles; neither construct being part of nature, though both are essential parts of the physical universe. It has been said that the operators in question are inadequate to define a system because they leave a large aspect of the construct undetermined; they are consequently, it is maintained, to be supplemented by further characteristics. This attitude, however, springs from the mistaken belief that a construct must be defined as completely as a datum of nature, which is clearly unnecessary if we recognize the essential distinction between these two kinds of entity emphasized previously. Let us see whether the older "definition" of an electron is complete. Can we say that the particle is truly round? Is it solid, that is, does it represent a volume distribution of charge or a surface distribution? The older theory permits an answer to none of these questions, although the tendency toward visualization will forever raise them. It is, I feel, a great advantage peculiar to abstract constructs that they do not suggest questions which presumably cannot be answered. In this sense we become more modest in our claims when we use the more abstract formulation.

Historically, Hamiltonian operators and momentum operators have developed from pseudo-sensible constructs of charge and mass, and it cannot be denied that the formal resemblance between the quantum mechanical Hamiltonian operator and the

so-called classical energy expression is extremely close. In fact we construct the former from the latter in nearly all practical cases, hence the claim that we are still operating with the old construct of a massive and charged particle. One can not deny that the quantities charge and mass are being retained in the new symbolism, which is therefore not entirely free from an admixture of sensible and pseudo-sensible constructs as indeed it should not be. We have already seen that all constructs, since they must permit of correspondence with data, possess necessarily some of the characteristics of nature, in particular they contain some allusion to space and time. The Hamiltonian operator does involve space and time coördinates; the fact that it also contains the constants m and e hardly renders it less abstract. The reader will recall that every abstract notion, if it is to be intelligible, implies some reference to sensory experience.

But as to the assertion that the abstract electron still represents a particle, our answer is different. The directions for finding the Hamiltonian operator from classical expressions have been termed a cookbook recipe, and this is a very appropriate jest. It brings out clearly the fact that, in passing from the constructs of classical physics to those of quantum mechanics, we are not *deriving* conclusions from premises, but we are making a transition which has been found successful. The starting point is a heuristic one; from a logical point of view it would be better to begin with the abstract constructs and deduce from them the others by showing how, for instance, in the limiting case of very many electrons, the abstract constructs permit a sensible interpretation. History, proceeding inductively as it always does, has inverted the logical situation. It is of course absurd to speculate about the sequence of discoveries if things had had a different start; but it is at least imaginable that the abstract formalism of quantum mechanics might have been constructed before the classical analogue was known, if mathematicians had looked more seriously for fields of application of their theories. In that event, the symbols e, m, V, appearing in the Hamiltonian operator H would probably not have been called charge, mass, and potential, and would have derived their significance merely

from the fact that, if they were inserted in H, and the mathematical crank was turned, the results agreed with observation. Even the functional form of V could conceivably have been determined by postulating conformity of results with data. The identification of e, m, and V with the classical quantities charge, mass, and potential of an electron would then perhaps have been a separate discovery. It is fortunate that we have a simpler way for ascertaining V, but this does not change the logical aspect of the matter. Quantum mechanics, we conclude, is not *logically* dependent upon the classical sensible notions; it is a discipline in its own right, which operates with abstract constructs. Heuristically, the most convenient way of arriving at these is via the familiar classical notions.

The abstract formalism avoids the question: what *is* an electron? To see how it manages to do this let us refer to an example which has already been mentioned, the photoelectric effect. The data may be described in a compact fashion by saying that, under the influence of light, negative charge emerges from a material surface. The explanation of this phenomenon amounts to this: We suppose a region of space (the interior of the material) to contain electrons, a term which is a label for a set of directions. These directions include a method for finding the charge at any point of space, a method which depends on the mathematical form of the Hamiltonian operator. If we calculate this charge at a point outside the material the result is zero. Next we wish to take account of the datum: presence of light. This changes the form of the Hamiltonian operator in a well known manner, and if we apply again the method for finding the charge after this change has been incorporated, the result at a point outside the material is no longer zero. Nothing has been said in this procedure about what an electron *is*, yet the result is entirely satisfactory. We are not, therefore, curtailing the physical universe by using this abstract mode of explanation; we are omitting constructs which, at least in the present state of knowledge, have no counterpart among data. We are subordinating our choice of constructs more effectively to the requirement of simplicity.

If the physicist were to restrict himself exclusively to the use of

sensible and pseudo-sensible constructs his dream of an ultimate system of explanation would never come true. This point argues against the view that physical reasoning should employ only mechanical models. We have seen in part IV that the correspondence between constructs and data must be an extensive one. The ideal system of explanation is one in which every quantity appearing among the constructs has its counterpart on the side of nature, thus rendering the initial correspondence universal and avoiding all breaks. Now it is a fact that every set of sensible or pseudo-sensible constructs, and hence every model, necessarily implies more than the data which it is designed to explain: it implies something which should correspond to its internal structure. Often this something is found, and the model, originally a unit, is divided into smaller mechanisms. The question as to the significance of the internal structure of the smaller mechanisms arises again; the difficulty has been pushed in the background but not eliminated. Continuity of space acquires a parallel in the never ending conquest of the physical universe.

This is of course precisely what has gone on in physics before the advent of recent theories. The original elements of Empedokles revealed themselves as a collection of atoms. For a very long time it occurred to nobody to worry about the structure of an atom, but finally it gave birth to electrons and nuclei. At present we are splitting up the former and wondering about the constitution of the latter. The fact that on the one hand the parts of which a nucleus consists are, so far as we can speak of size, about as large as the entire nucleus, and the amazing failure of finding any empirical clue to justify the assumption of a coherent electron on the other, adds material evidence to the formal difficulty of never ending subdivision. The introduction of abstract constructs, giving rise to spatio-temporal indeterminacies as we shall see, terminates this process of dissection.—This argument for abstract notions is by no means conclusive; yet it confers upon them a quality which makes us regard them with favor at present, and gives us the stimulus to try them out. No one can say that in the future their use may not be abandoned.

VII. ELEMENTARY DIFFUSENESS AS A RESULT OF ABSTRACT PHYSICAL CONSTRUCTS

At the end of section III we suggested a division of constructs into two groups: physical systems and quantities characterizing physical systems. In that connection attention was called to certain combinations of systems and quantities, called states, but the exact manner in which these combinations are made was not examined. In the preceding section we have introduced a different classification of constructs from a more qualitative point of view. This classification will aid us, however, in obtaining a deeper insight into the meaning of physical states.

Sensible and pseudo-sensible constructs have their origin, at least partly, in the domain of the physical universe here called nature. It is not surprising, therefore, that those physical theories which employ, or claim to employ, only these two classes of constructs imitate in their methodological formalism the conventional analysis of data. Now data are usually conceived of as *things* with definite *properties*, the relation between properties and things being one of possession. In the field of symbolic construction this relation has found its parallel in the direct assignment of quantities to systems, in a possessive sense. Thus it comes about that theories which abstain from the use of abstract constructs formulate a state in just this manner, namely by ascribing to a physical system a sufficient number of quantities, each of definite amount. In mechanics, the state of a particle is described by specifying its position and its momentum.

All quantities, through their epistemic definition, are measurable, and the result of a measurement is an arithmetical number, or a range of numbers. Hence a state formed in the manner under discussion can always be symbolized as a group of numbers, and as the state changes, the numbers in this group vary. Calculations made on the basis of this scheme involve these numbers; they contain none of the more abstract mathematical apparatus except in so far as they facilitate the treatment of numbers. The correspondence between constructs and data is of a character that assigns to a number or group of numbers forming the result

of a calculation, a definite number defining the result of an experiment.

For the sake of completeness and precision these remarks should be qualified somewhat. Observations are not exactly repeatable, hence we can not speak significantly of a single number as defining the result of an experiment. But there is a method, developed by means of the theory of errors, which permits the assignment of a single number to a series of slightly divergent observations. It is this number to which our statement referred.

When no effort is made to limit constructs to the pseudo-sensible type one meets with an altogether different situation. The systems themselves, as we have seen, no longer partake of the attributes of data, and quantities can not reasonably be assigned to them possessively, as before. To be sure, physical quantities, the only measurable kind of construct, must in some way be brought into the picture because they form the last link in the correspondence between constructs and data. But since they need not be assigned to systems in as direct a manner as before we can proceed more freely in the composition of states. In particular, any reason for expressing a state in terms of a group of arithmetical numbers has ceased to exist. We may, if we wish, bring in the entire machinery of abstract mathematical construction, provided we make sure that a transition from the selected states to physical quantities, and thence to data, is possible. Since data are dominated by arithmetical numbers, this transition must ultimately lead to numbers, but not of necessity to a single number, for it may well be that our theories determine data in terms of aggregates of numbers, that is, statistically.

The preceding paragraph sketches a general outline into which such theories as quantum mechanics fit quite accurately. States are here defined by means of *state functions*, functions of space and time coördinates which make no explicit reference to quantities. Only in their choice of space coördinates do they reflect the abstract properties of the system whose state they represent. Yet while they do not suggest quantities directly they are related to them through well known rules of operation. Suppose, for instance, that an atom is in a state symbolized by the state

function ψ $(x_1, \ldots . x_{3n}, t)$, n being its number of degrees of freedom.[14] Then, if we wish to find the value of the quantity: total energy, we perform the operation $\int \psi^x H\psi dx_1 \ldots . dx_{3n}$, where H stands for the Hamiltonian operator appropriate to the atom. But it is found that the number which results upon integration, while it refers to the total energy, does not in general correspond to any single number among the data obtained by measuring the energy of the atom; instead, it turns out to be their *weighted mean*. This was meant when we said that the relation between states and quantities is less direct. It is possible to get even more information from the state function, for we can, by simple expansion of ψ in a series of well defined functions known as "eigenfunctions of the energy," or "pure states with regard to energy," find the probability that, in a given measurement, the result will be a preassigned number. In this sense a state function implies, not specific values of a set of quantities, but statistical aggregates of such values. The example here chosen is typical of all other quantities. I have given fuller details of the procedure in an earlier paper.[15]

Although the constructs employed in quantum mechanics appear far removed from the realm of nature, the results to which it leads are in better accord with data. For physical data, at least in their cruder state (i.e. before the divergence of the individual observations has been removed by special procedures) do form statistical aggregates and not single numbers.

It should be observed above all that the requirement of causal description, discussed in section III C, has not been dropped from our methodology. The physical states, to which alone this requirement refers, are still governed by unique laws of temporal variation. In the present case, the state functions obey a first order differential equation in the time, and if ψ is given at any instant of time its later or earlier form can in principle be calculated with precision. Data, however, are no longer exactly

[14] n need not be thought of as number of "particles" multiplied by 3; in fact the number of degrees of freedom, if we desire to retain the term in this connection, should be regarded as a suitable number ascertained by trial.

[15] H. Margenau, The Monist, 42, 161 (1932).

predictable. Another point worthy of special emphasis is this. The recent theories, exemplified in the preceding paragraphs, do not fall outside the frame of the methodology which has here been developed. It has not been necessary to recast the fundamental principles of physical reasoning in order to bring them into line. By placing into prominence certain abstract features which have always been inherent in physical explanation it is possible to reconcile apparently divergent tendencies among the various theories of physics, and to restore to the physical world the uniformity which it threatened to lose.

Let us not forget, however, the change in the manner of predicting elementary data which is consequent upon the use of abstract constructs. We have shown how the greater degree of freedom in connection with the definition of states permitted by them elevates us above the necessity of numerical description, and how this, in turn, makes possible a peculiar diffuseness in the data to be represented by the theory. To put it another way, if we were to design a theory after the classical fashion, i.e. one whose constructs are all pseudo-sensible, which would predict similarly diffuse data, then *such a theory would be acausal.* This is the precise meaning of the famous complementarity principle formulated by Bohr and advocated so strongly by Heisenberg. Two special instances of what has here been called elementary diffuseness are of great importance. One is known as Heisenberg's indeterminacy principle; it has been discussed extensively both in the physical and in the philosophical literature—though often very superficially—hence we may here be justified in dismissing it without further comment. The other has to do with an interesting modification in the concept of a "pure case." This we wish to describe briefly.

The customary notion of a pure case as that of a (constructional) situation in which a multitude of systems, while differing in the assignment of the amounts of certain quantities, all possess the same amount of one particular quantity. Thus a group of small magnets (for instance atoms) flying through space with different velocities, different energies, each being, of course, at different points of space, might have their magnetic moments

all oriented along a certain axis. This group is then said to represent a pure case with respect to orientation of magnetic moments. (The example here chosen is of concrete importance, since the corresponding experiment can actually be performed. Cf. any textbook on the Stern-Gerlach Effect.) The test as to whether or not such a pure case is present would be to pass the stream of atoms through a suitable magnetic field devised to stop all atoms which do not have their magnetic moment along the direction in question. (We need not remind the reader that we are here using the customary technical lingo which obscures distinctions between constructs and data.) Now the interesting point is this: No matter how hard we try to bring about the state of perfect homogeneity regarding the direction of magnetic moments, some atoms will always be stopped. Evidently, the attainable degree of homogeneity is not the maximum imaginable, it is distinctly smaller. There is an inevitable diffuseness in the orientation of moments, which follows clearly from the abstract definition of states. This example may in fact be regarded as partial empirical evidence for the superiority of the use of abstract constructs.

VIII. SCIENTIFIC PROCEDURE

We have concluded the search for the object of physical inquiry and thus disposed of the major and more problematic part of our task. Though the working physicist is not in general fully conscious of the rules of the game which we have outlined, he allows himself to be guided by them instinctively through a sense of fitness he has acquired in his training. This background of method is the birthplace of his problems; failures of data and theories to conform, in the manner of this scheme, provide the principal urge for his researches. And while it is true that valuable achievements are often inspired by other motives, whether or not they are to be incorporated into physics is always decided by reference to this methodology. The results of electrical engineering are not counted as physical issues unless or until they enmesh themselves in the relations and requirements here set forth.

Yet these rules are not to be regarded as a program to which physical activities must always conform; for then our methodology would be final, which as we have pointed out, it can not be. Rather, the rules are the results of our attempt at crystallizing what the physicist is doing. Hence their normative value is not absolute or enduring. Their exposition is important, however, because the development of physical science is not likely to be shifted suddenly from its accustomed course, so that the older rules of guidance may well be applied to new unruly theories. One should certainly not abandon time-tested principles without compulsion.

To complete our task let us consider briefly *how* the physicist solves his problems, that is, how he fits his details into the abstract methodological scheme. The latter involves a formal bifurcation of the physical universe into nature and the domain of constructs; to this there corresponds the usual division of physical activity into experimental and theoretical research. In so far as we have, in some of our examples, drawn a sharp line of demarkation between nature and constructs, our considerations may have appeared forced from a practical standpoint, for such a line does not conventionally exist. In principle, however, it can and should be constructed. In actual investigations it is often impossible to maintain a sharp boundary between practical and theoretical procedures, because the manner in which we explore nature is already dependent upon available constructs. Psychologically, but not logically, the sharpness of the division is an illusion. Thus it would be difficult to point out a simple experiment that does not utilize theory in some form.

Experiments can be performed with two different ends in view corresponding to the fact that nature is both start and terminus of every complete cycle of physical inquiry. Those experiments which establish a start are usually excursions into a new range of phenomena; in the majority of cases they are conducted by accident. The other, more common type, is designed to test a theory, that is, to confirm or disprove the extensibility of a set of constructs of explanation. The results, if confirmatory, are then said to have been predicted by the theory. To this cate-

gory also belong the experiments which are performed to decide between several conflicting modes of explanation.

Turning now to theoretical physics we may ask: How do we go about the business of constructing theories? This question could be dealt with at considerable length, but I do not wish to burden this already comprehensive paper with incidental details. There are three main procedures, and each could be illustrated by numerous examples. The first is to begin with an internally coherent but detached system of constructs and work out the data which it should entail; but this is rarely if ever done at present since we are already in possession of too large a collection of constructs. The second is the reverse of this, namely to allow data to suggest at once a formalism of explanation closely in line with observations. This is possible and indeed common in fields where the prominent constructs are sensible or pseudo-sensible, but of little avail in others. Large scale mechanics has been developed by the use of this procedure. Finally we come to the method most widely employed. An existing set of constructs which fails to correspond correctly to an enlarged domain of nature is modified by generalization, that is by removing qualities of constructs which, though they slipped inadvertently into their original definition, are not required in accounting for any phenomenon observed. As a result of this progressive removal of obnoxious hidden properties modern physics has a tendency toward abstractness and assumes increasingly the character of mathematical analysis. The revision of the constructs of time, space, and mass in the special theory of relativity illustrate the point. Independence of these entities had unwittingly been included by Newton in their definition, chiefly, I suppose, to render them sensible. When Einstein replaced this independence by suitable relations more general than the assumption of absolute constancy, he was able to make these modified constructs represent a far larger domain of physical data.

IX. REFLECTIONS UPON CONTEMPORARY THEORIES

As a supplement to the main theme of this paper I wish to add a brief and non-analytic exposition of two very recent theories,

which, by their strangeness, have caused considerable consternation; our aim being to determine more exactly those features in them which stand in contrast to present methodical requirements. In the account that follows we shall exercise little care in distinguishing constructs from data; for if a physical situation were described with all the fullness of its logical and metaphysical implications, the result would be as tedious as a story in which each word is paraphrased by interpolations calling attention to the particular shade of meaning to be conveyed.

The first is Dirac's theory of the positron, the newly discovered "particle" having a mass equal (within an experimental uncertainty at present quite large) to that of the electron, and a charge equal in magnitude but opposite in sign to that of the electron. Our story starts with Schrödinger's quantum theory, in particular its application to an electron. The reader probably knows that this theory explains in a striking manner the behavior of the negative element of charge both when it is nearly free, as in the experiments on electron diffraction (Davison and Germer), and when it is bound in atoms or molecules. Most spectroscopic data are beautifully accounted for on the basis of its formalism. Nevertheless there were peculiar difficulties when observations were carried to sufficient detail. Spectral lines which according to the theory should be single turned out to be doublets on closer examination; and when lines were emitted by atoms subjected to a magnetic field (Zeeman-effect) they exhibited a complexity of which the theory could give no account.

In fact these difficulties were known before the development of Schrödinger's theory, for Bohr's theory, the predecessor of present quantum mechanics, had similarly failed to explain these anomalies. But in connection with the latter the discrepancies had been removed by the brilliant hypothesis of Uhlenbeck and Goudsmit, who assigned to the electron—in Bohr's theory still a pseudo-sensible construct—a motion of rotation, now called a spin. The literal transcription of such an hypothesis into Schrödinger's theory was impossible, for there the electron had lost its sensible character. But something similar could be done: an additional degree of freedom could be assigned, in a formal manner, to the

electron, and by revamping somewhat artificially the Schrödinger theory it could be forced to give correct results. Pauli, in his theory of the spin, succeeded in achieving this end. In a fundamental sense, however, the difficulty still loomed large.

It occurred to a number of investigators that Schrödinger's theory might be at fault because it does not satisfy the requirements of the relativity theory. Various attempts were made to remedy this defect, mainly by adding terms whose effect was inappreciable when the velocity of light was considered large, so that, in the limit of an infinite velocity of light the Schrödinger equation was restored. This procedure yielded some interesting results but did not obviate the difficulties for whose removal the spin had been invented. The spin still had to be tacked onto the theory in the manner of an afterthought in order to make its predictions correct.

This unsatisfactory situation was greatly relieved when Dirac succeeded in his attempt of reconciling quantum theory and relativity. Instead of producing relativistic invariance of Schrödinger's equation by superficial means he recast the entire formalism on the basis of novel considerations that are logically very simple indeed. His modification amounts in fact to *two* improvements: 1) The insurance of the correct relativistic behavior of the equations; 2) the establishment of a correct statistical interpretation.[16] When this was done the spin was no longer needed; the new theory produced automatically all the data which Schrödinger's equation and the spin theory jointly had predicted. Physicists had every reason to rejoice over this unification which represented a triumph of the same order as the consolidation of constructs attained in the relativity theory itself. The implausible spin, as we pointed out, had vanished from the picture. The term, to be sure has been retained. Even now physicists make calculations in the basis of the older theories in which the spin figures as a separate entity, but this is done merely for the sake of convenience. The mathematics of Dirac's theory is sometimes

[16] To this end the differential equations were written in first order form, which forces statistical weights to be always positive. The second order form would admit such absurdities as negative probabilities.

difficult—a discipline of great logical simplicity is often forced to employ unfamiliar kinds of analysis—but it can be shown that it reduces to the form of Pauli's spin theory in certain limiting cases. Hence, when we are dealing with these limiting cases, we avail ourselves of this opportunity to simplify the computations.

Unfortunately, the happy state of affairs was soon disturbed. Upon closer inspection it was seen that Dirac's theory made a perfectly grotesque prediction. It implies the occurrence of electrons with negative kinetic energy, or negative mass. Translated into terms of data, there should be ionizing agents (as manifested in cloud chamber photographs) which grow more effective as they are opposed by matter, loosely speaking, there should be particles which go the faster the more energy they lose; under an attractive force they should be repelled. Not only are such data unobserved, they would also disqualify the entire theory of Newtonian mechanics by invalidating the law of action and reaction. We are confronted with a case where the correspondence between a set of constructs and certain parts of nature can not be extended to other parts, a situation with which we have already dealt in a general way. This limitation of theories, as we saw, is frequently encountered, and a theory is not always rejected if it appears. Hence, one might say, let us set an arbitrary limit to its validity by saying that Dirac's theory can not be applied to make such monstrous predictions in the same sense that Hooke's Law, for example, must not be applied to inelastic bodies.

This escape is blocked, however. For on striking out the negative kinetic energy states the Dirac theory collapses. Its mathematical structure is founded essentially upon these states (state functions do not form a "complete set" without them); furthermore, their existence is already presupposed in some of the successful applications of the theory (Klein-Nishina formula). Hence, what are we to do?

At this point Dirac made a sweeping suggestion. Instead of denying the occurrence of these obnoxious electrons he postulated their universal presence. According to the Pauli exclusion principle every energy state can "contain" at most two electrons. Let us now suppose that almost all negative kinetic energy states

be filled and that the incumbents of these states be *unobservable*. The vacancies in this distribution of negative electricity are then observable and exhibit the normal properties of an electron with positive charge, as can be shown analytically. If these "holes" are rare the stability of the ordinary, i.e. negative electrons with positive mass is insured for they can not convert themselves without limit into the unobservable paradoxical kind because of the lack of vacancies in their distribution. Thus Dirac was led to the prediction of the positron even before it was discovered.

But the story is not yet complete. If there are unoccupied energy states among the undesirable electrons, an ordinary one may occasionally pass into one of these states by changing its energy from a positive to a negative value. Since the unoccupied state represents a positron, this process is equivalent to the simultaneous annihilation of an electron and a positron and the emergence of energy in the form of radiation. A mechanism of this sort has often been postulated in connection with cosmic processes and is not unlikely to be operative. But the reverse process, i.e. the removal of an electron from a negative kinetic energy state to one of positive energy, which by the same interpretation should correspond to the creation of a pair (positron + electron) with accompanying absorption of energy, is manifest in numerous cloud chamber photographs and agrees with observations even in many quantitative respects. These distinct successes make it difficult to look upon Dirac's proposal as an idle speculation.

In what way does the theory fail to satisfy the requirements outlined in this paper? Its constructs are abstract, which, as we now know, is no comment on its value. The correspondence between its symbolism and nature is very widely extensible as the foregoing remarks suggest. But it falls down in the point of simplicity because of its introduction of constructs to which, *by definition*, no data shall correspond. In this respect the theory recalls somewhat the abortive attempt of explaining the necessary mechanical properties of a luminiferous ether by installing in it a multitude of invisible gyrostats. One may well feel that Dirac's theory is incomplete in its interpretation and use of the negative kinetic energy states, but it seems unreasonable to suppose that it is altogether misleading.

Dirac's hypothesis is not the only recent proposal for eliminating paradoxes by inventing constructs without counterpart on the side of nature. Fermi's suggestion regarding the treatment of the continuous beta-ray spectrum is another noteworthy example. The difficulty is this. Some radioactive elements are known to emit electrons, which appear to have their origin in that very limited region inside an atom called the nucleus. It is quite difficult, in the first place, to think of an electron as being enclosed in the nucleus, but if we must place it there, as the observations suggest, then its energy must be "quantized," i.e. it can not have any energy it pleases, only one or another of a discrete set of values. This, at least, is the customary argument (which could be criticised). Now the emitted electrons have a continuous distribution of energies, and this is not because they have lost a portion of their original energy by encounters with extra-nuclear electrons as has been shown experimentally. Thus there is evidently a certain amount of energy which slips through the net, for a discrete energy distribution can become continuous only by subtraction or addition of energy, and the latter possibility is in this case excluded.

A trivial way out of the difficulty would be to abandon the principle of conservation of energy, a very radical step which has often been discussed. It is not necessary, of course, to question the constancy of the total amount of energy in the universe for the sake of these data; they only suggest that the principle may not be applicable in detail to single elementary phenomena, whilst in the aggregate processes manage to balance energetically. In my opinion such a proposal has very little force indeed. In the first place, if it is taken literally, it means simply that the elementary processes take place in systems which are not closed, in other words, that there are interactions between the nuclei, or different parts of one nucleus, which the theory is neglecting. Thus the problem is not solved, it has only shifted its ground in so far as we must then look for these unknown—and implausible—energy exchanges. But the suggestion can not be taken literally, and this is the second, more important point we are making. The suggestion has its origin in a confusion of concepts which is very common. We know that atoms and electrons, from the

quantum mechanical point of view, are abstract constructs to which quantities, like energy, can not be directly assigned. To be sure, we continue to speak of them as though they were pseudo-sensible, principally to avoid cumbrous discourse. Unfortunately, this ambiguity of speech has entailed an ambiguity of thought in as much as physicists are tempted to cling to the picture of particles. The suggestion that energy be not conserved in detail can only be made by one who succumbs to this temptation; for quantum mechanically it is perfectly clear that energy need not be conserved in a single observation. If the system, in this case the electron, happens to be in a state which is not an *eigenstate* of the energy, this quantity is only statistically determined anyway, and one fails to see the meaning of the proposal.

An alternative explanation of the continuous beta-ray spectrum involves the assumption of unobserved entities, called neutrinos. Crudely, one thinks of them as particles capable of carrying away energy. If one lets them fly off simultaneously with the observable electrons the energy balance can, of course, be established. Fermi has recently given quantitative precision to this thought. One might suppose at first sight that such a theory must be highly indefinite and could prove nothing. Yet it is surprising how, in Fermi's able analysis, the results become well shaped and definite. It turns out that, if negligible (rest) mass and no charge are assigned to the neutrino and the latter is described by Dirac's theory (which is known to be applicable only to electrons), we can account not only for the existence of continuous beta-rays, but also for their distribution-in-energy.

Methodologically, this hypothesis sins against the requirement of simplicity in the same way as did the former, the only difference being that the neutrino is not in principle unobservable, but merely unobserved. Hence the proposal is capable of empirical proof, theoretically. However, the assumed properties of the neutrino place it in a position practically beyond refutation, for it must be difficult to detect a particle without mass and charge. In one sense, the hypothesis of Fermi is much more violent than Dirac's. While Dirac postulates only *one* kind of unobservable entity, Fermi implicitly assumes *three*. If the neutrino is to be governed by Dirac's equation, negative

kinetic energy neutrinos are automatically called into being, and if one wishes to dispose of them by Dirac's famous trick of filling all possible energy states with them and then invoking the exclusion principle, he must account for eventual vacancies by admitting the existence of highly paradoxical entities which could be called "anti-neutrinos."—Fortunately there is little reason to be alarmed at these monstrosities, for we have already indications of a more technical kind that Fermi's hypothesis is at odds with other physical data.

In concluding I desire to make two points. First I wish to answer the critic who maintains that the division of the physical universe into constructs and data, which underlies the entire methodological scheme here presented, is artificial, unnecessary, or misleading. I suggest to him that he analyze logically, and without metaphysical bias, a simple proposition of the form: Light is an electromagnetic disturbance, and see if he can get along with less than two classes of things. The position here outlined is not founded upon philosophical convictions; it results from an attempt to classify physical concepts in the simplest possible and at the same time most inclusive manner. I have tried to analyze matters from a more definitely positivistic point of view but have not succeeded.

Secondly, I can not admit that the stand here taken with regard to the abstractness of physical constructs is too radical, nor that abstract notions could not be ultimate. If anything, we have erred in the other direction, when we claimed that all constructs, including the abstract ones, must permit some reference to space and time, these forming the substratum of nature. Laue[17] and Schrödinger[18] have recently advocated that space and time may lose their significance in connection with ultimate constructs, thus opening an avenue to even greater freedom than we have here permitted.

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[17] M. v. Laue, Naturwissenschaften, 20, 915 (1932); 22, 439 (1934).
[18] E. Schrödinger, Die Naturwissenschaften, 22, 518 (1934).

Samuel Alexander's Concept of Space-Time

BY

HARRY RUJA

SINCE the seventeenth century, when Descartes divided the universe into two realms, the distinctive character of one of which being its spatiality or extendedness, philosophers have been increasingly concerned with the analysis of the category of Space. Scientists, too, have been interested in the concepts of Space and Time, especially since the promulgation of the theory of relativity.

Samuel Alexander, the contemporary British philosopher, is one of those who have thought it desirable and important to subject the concepts of Space and Time to a close and critical scrutiny. Although undeniably influenced by recent physical theory, his interpretations, embodying as they do the philosopher's interest in the most universal aspects of reality, stand largely on their own feet. His *Space, Time, and Deity*, which C. D. Broad has characterized as "a great work of philosophic speculation, nobly conceived and conscientiously carried through,"[1] erects the concept of Space-Time into the most fundamental category of reality. His book is primarily metaphysical, although some epistemology does enter. The central rôle which Space-Time plays in Alexander's system may be judged by the following quotation:

"It is not," he says, ". . . too much to say that all the vital problems of philosophy depend for their solution on the solution of the problem

[1] *Mind*, XXX, (1921), p. 150.

what Space and Time are and more particularly how they are related to each other. . . . Space and Time are the simplest and most fundamental characters of the world."[2]

And it is this fundamental concept of Space-Time which Alexander is concerned to interpret in *Space, Time, and Deity* and whose influence is, he thinks, so ubiquitous and important.

And it is his interpretation of this concept which I am concerned in this paper to describe and, to an extent, to criticize. This paper will, accordingly, be divided into two parts: (I) Exposition, and (II), Criticism. In the first part I shall describe the mode of apprehension of Space-Time as Alexander conceives it and the main general characteristics of Space-Time as Alexander delineates them. In the second part I shall criticize Alexander's treatment of three concepts: (A) Quality, (B) Universals, and (C) Error and shall attempt to show that a naturalistic universe consisting only, basically, of Space-Time can not adequately provide for these three entities.

I. EXPOSITION

A. *The mode of apprehension* of Space-Time, Alexander does not even pretend is sensory. In fact, he explicitly asserts that it is not. The things in space may be known through the senses: Space itself cannot. "We have not any sense-organ for Space or Time," he says; "we only apprehend them in and through our sensible apprehension of their filling."[3]

Here is some of that epistemology which, I said, unavoidably intrudes into the otherwise untainted metaphysical sublimities of *Space, Time, and Deity*. Knowledge, for Alexander, is not a prerogative of the sense organs. In fact, knowledge is not an attainment of the sense organs at all. Knowing occurs *through* the sense organs—not *by* them. In the last analysis, we know through "intuition," meaning by intuition not anything mystical and ineffable but merely a "form of apprehension simpler than

[2] S. Alexander, *Space, Time, and Deity*, "The Gifford Lectures at Glasgow for 1916–1918," Two Volumes, Macmillan & Co., London, 1920; Vol. I, p. 35, Vol. II, p. 182. Unless otherwise designated, it is this book to which all future references in this paper refer.

[3] I, 37 (volume and page number).

sensation" and fundamental to it.[4] "Half our difficulties have arisen from attempting to regard Space as given us by touch or sight instead of only through touch and sight."[5] We do not sense Space-Time as we do the trees and houses and tables in it. But we do, none-the-less, know it certainly and assuredly.

This dismissal of sensation as a medium of knowledge of Space-Time is not to be taken as vindication of the supra-rationalistic-Parmenidean-Bradleyan manner of dictating to Nature and demanding that she conform to men's prejudices. Metaphysics is, legitimately, a descriptive science. It can claim respect as knowledge insofar as it conscientiously tries to know and disavows dictation. "Philosophy proceeds by description; it only uses argument in order to help you see the facts just as a botanist uses a microscope."[6] We need to sit humbly before the magnitudes of Reality and seek to know it as it is. Arrogance is intellectual suicide.

But with the proper attitude, we can know it. Alexander is no skeptic; for skepticism spells nihilism, and Alexander refuses to flounder into nothingness. We can know and we do know. All Alexander is concerned to maintain is that knowledge is not equivalent to sensation. But knowledge, fortunately enough, does occur.

And that is why Alexander does not do as some philosophers of science do (*e.g.*, A. C. Benjamin) and differentiate mental Space and Time—the Space and Time given us by crude, uncritical experience—and physical Space and Time—the result of sophisticated analysis.

For Alexander, there is only one Space and Time: viz., the Space and Time of experience. It may be that sensation gives us a different Space-Time from that upon which we finally decide; but sensation is not a dependable guide: sensation is not knowledge. And for purposes of knowledge, it is not important or necessary to consult the Space-Time which crude sensation gives us.

[4] II, 147.
[5] I, 153.
[6] Alexander, "Some Explanations," *Mind*, XXX, (1921), 423.

A possible distinction can be made between mental Space-Time and physical Space-Time as a consequence of a certain psychological distinction which Alexander makes: that distinction is the distinction between "enjoyment" and "contemplation." An *act* of awareness is "enjoyed"; the *object* of awareness is "contemplated."

Thus one could distinguish mental and physical Space-Time by saying that the former is that enjoyed by mind while the latter is that contemplated by mind. "By mental or psychological time," Alexander says, "I mean the time in which the mind experiences itself as living, the time which it enjoys; by mental space I mean . . . the space in which mind experiences itself as living or which it enjoys."[7]

But even this distinction is provisional and tentative. " . . . mental space and time possess the same characters . . . as physical space and time, and . . . mental time . . . is a piece of the Time in which physical events occur; and similarly of mental space."[8] "If we are asked the question what do you mean by Space and Time? Do you mean by it physical Space and Time, extension and duration, or mental space and time which you experience in your mind . . . or do you mean by it the order of relations which mathematics investigates? The answer is, that we mean all these things indifferently, for in the end they are one."[9]

And hence it is that only one Space-Time will be described—the Space-Time which experience gives us. Alexander styles himself a "thorough-going" empiricist,[10] and ransacks the deliverances of experience for his data as to the character of Space-Time. This, then, is the mode of apprehension of Space-Time.

B. What are some of the *main general characteristics* which, according to experience, accrue to Space-Time?

1. Well, in the first place, Space-Time is *metaphysically real.* No Kantian subjective forms of the mind for Alexander! And no Bradleyan illusions of the finite mind for him either! Space-Time is neither a subjective form nor an illusion. It is the most

[7] I, 93.
[8] *Ibid.*
[9] I, 180.
[10] I, 165.

real and the most concrete thing in the universe. Indeed, its reality is almost a mere supposition with him. "We do not ask whether they are real in their own right or not but assume their reality, and ask of what sort this reality is."[11] Space-Time is objective and absolute. Kant would have been right if he had *not* asserted space "to be a subjective form."[12] Space-Time is as objective as anything can be.

And this gives objectivity to the sciences of Space-Time. This makes geometry "a basal science of reality"[13] and metaphysics "the most concrete of all the sciences."[14] For what could be more concrete than the sciences of the most concrete thing in the world, Space-Time?

2. And, secondly, it is Space-Time—not Space alone or Time alone. Space and Time are *indissolubly intertwined* and vivify each other. "There neither is Space without Time nor Time without Space . . . Space is in its very nature temporal and Time spatial."[15] Disjuncted, Space and Time would be vitiated. Particularly damaging would the disjunction be on the continuity of Space-Time, a property which I shall consider later.

"If Time existed in complete independence and of its own right, there could be no continuity in it. For the essence of Time in its purely temporal character is that the past or the earlier is over before the later or present. The past instant is no longer present but is dead and gone. . . . The mere temporality of Time, its successiveness, leaves no place for its continuity or togetherness and seems to be contradictory to its continuity. . . . Time cannot be what it is without indissoluble relation to Space."[16]

Space, too, requires Time for its fullest realization. Without Time, Space would be a mere undifferentiated blank. There is no such thing as empty Space: if it contains nothing more, it contains at least Time. And, *mutatis mutandis*, the same holds

[11] I, 35.
[12] I, 39, note 1.
[13] I, 152.
[14] *Mind*, XXX, 410.
[15] I, 44.
[16] I, 45, 46, 47.

true of Time. "All Space in fact is full of Time and there is no such thing as empty Space; all Time occupies Space and there is no such thing as empty Time."[17]

And hence it is that Space is not to be viewed as string of cross-sections at different instants. Such a Space would be an impossibly disjointed one: it would be impossible to get from one section to another. A Creator would be required who would recreate anew the entire universe at each new instant. Space is an undifferentiated blank without Time, and Time is a chaotic disjointed jig-saw puzzle without Space.

It is the fact that Space and Time have the two characteristics both of continuity and of discreteness that requires their union. Space alone provides continuity but neglects discreteness. Time alone provides discreteness but slights continuity. "Without Space there would be no connection in Time. Without Time there would be no points to connect."[18] Only a copulation of the two can resolve the dilemma and enable *both* continuity and discreteness to have real existences—as they are experienced to have—integrally in Space-Time. Space alone does not exist; Time alone does not exist; only Space-Time is real.

Put into other terms, this means that *events* are the primary "things" of reality. "Point-instants" are the real units of the universe. "There is no instant of time without a position in space and no point of space without an instant of time. . . . There are no such things as points or instants by themselves. There are only point-instants or *pure events*."[19]

Put into still other terms, this means that the universe is a universe of motions—of activity. Everything partakes in the procession through Space-Time. Dynamism is not imposed artificially from above; it is integral to the very constitution of the universe. "The real existence is Space-Time, the continuum of point-instants or pure events."[20] In Alexander's apt phrase, the universe is thru and thru "historical."[21]

[17] I, 145.
[18] I, 48.
[19] *Ibid.*
[20] *Ibid.*
[21] I, 65.

"Rest, in fact, appears to be purely relative and to have no real existence. Every place has its time-coefficient." In fact, a point anywhere in perfect rest would dislocate the whole system of Space-Time. "If anything could be absolutely at rest everything must be at rest. For if any point in space retained its time, this would dislocate the whole system of lines of advance within Space-Time, a point being only a point on such a line."[22]

Space, then, is completely "saturated" with Time; and Time is thru and thru spatial. Space and Time are in indissoluble union in Space-Time.

3. But what more precisely is the nature of this Space-Time? And here is Alexander's very original contribution to metaphysics. Space-Time, says Alexander is a *stuff* out of which all things are composed and which penetrates into every corner of the universe. True, Space-Time fulfills the function of a receptacle—it is the container in which all things have position. But it is more than a container. It is literally the stuff of the universe. Substances—the hills and the flowers and the houses and the men—are "nodosities" in Space-Time. Space-Time as such is perfectly uniform. But here and there, there are hillocks or ripples—things are no more than these ripples in Space-Time. All things are complexes of Space-Time.

The elements of Space-Time, says Alexander, recapitulating, "are represented conceptually as point-instants or bare events; and we have added the hypothesis that other empirical things or existents are groupings of such events, whirlpools within that ocean, or they are crystals in that matrix. Only whereas a crystal may be separated from its matrix, existents never can; they remain swimming in the medium of Space-Time."[23]

In their simplest terms, things are "differentiations" of Space-Time. Whether a thing is nothing but its "simplest terms" is to be seen later.

With regard to Space-Time itself, it does not make sense to ask whether it is a substance. It is the ultimate in terms of which

[22] I, 84.
[23] I, 183.

everything is to be resolved; but it itself cannot further be resolved. "Just as a roll of cloth is the stuff of which coats are made but is not itself a coat, so Space-Time is the stuff of which all things . . . are made,"[24] but not itself a thing.

4. Furthermore, Space-Time is *ubiquitous*, universal, and all sufficient. There is no supernaturalistic realm beyond the spatio-temporal realm either of the rationalistic or of the mentalistic, theological type. There is nothing beyond nature. This is Alexander's "naturalism."

There are no gaps in Space-Time, for that gap itself must be a bit of Space-Time.[25] The universe is chock-full of Space-Time. There are no holes in it. That does not mean that it may not be empty of qualitative events. "Space and Time may be empty of qualitative events or things, and if we are serious with Time there is no difficulty in the thought of a Space-Time which contained no matter or other qualities but was, in the language of Genesis, without form and void before there was light or sound. But though empty of qualities Space and Time are always full. Space is full of Time and Time is full of Space."[26]

Even the past and the future exist in some good sense in Space-Time. "Since existence is occupancy of a space-time in exclusion of other occupancy and since such occupation is always temporal, existence must not be limited to present existence but includes past and future."[27] And mind and quality and relations can also similarly be shown to require no other form of being than spatio-temporal.

The final unequivocal statement of the ubiquity of Space-Time is this: "Everything is a complex of space-time and possesses the fundamental properties of any space-time, which are the categories,"[28] and this: "All things, no matter what their qualities, are bits of Space-Time."[29]

[24] I, 341.
[25] I, 65.
[26] *Ibid.*
[27] I, 200.
[28] II, 71.
[29] I, 223.

5. The next characteristic of Space-Time has already been implied in the statement that Space may be full even when empty of things. That is, Space-Time is *independent of its contents*. Just as it is metaphysically real—independent of any mind which knows it; so is it ontologically real—independent of any thing which occupies it.

There may be some difficulty in so examining Space and Time in abstraction from the events and objects which they contain. "We are not accustomed to think about Space and Time themselves but about the things contained in them;"[30] primarily, probably, because "we have not any sense organ for Space or Time; we only apprehend them in and thru our sensible apprehension of their filling."[31] But this merely once more indicates the ineffectuality of mere sense experience. Despite its difficulty, so must we think Space-Time—independent of its filling. For that is how Space-Time *is*—independent of its filling.

6. Another characteristic of Space-Time is its function as the *medium of duration and extension*. It is more than a receptacle. But it is a receptacle. "Space and Time . . . are what are commonly known as extension and duration, entities . . . or forms of existence in which bodies occupy places and events occur at times or moments."[32] This characteristic is a familiar one and should meet with no questionings; so I move on.

7. Space-Time, seventhly, is a *continuum*. That is, while it is composed of discrete units (point-instants), its units are so distributed that between any two units a third can be inserted. Furthermore, it has no last or first term—*i.e.*, it is infinite.

> "Space is infinite because it is self-contained. Choose any selection of its parts accordings to some law of selection and you find that that selection is itself infinite and contained within the original. . . . Between any two members another can be found."[33]

Its continuity makes motion possible. For if it were true that between one point and the next, there was a barrier of non-

[30] I, 37.
[31] *Ibid.*
[32] *Ibid.*
[33] I, 43.

being, motion and change would be impossible. An entity would cease to exist after it had left its point in Space and would need to be recreated upon "its" arrival at the new point. Such antinomies are obviated by making continuity a central integral character of Space-Time.

8. Finally, Space-Time is perfectly *uniform* in all directions. It has no snags in it. All over it is perfectly homogeneous. As Alexander prefers to express it, Space has a "constant 'curvature'."[34] And as a result, mere movement in Space-Time does not affect an entity. A round ball does not become elliptic just because it is moved from one corner of the room to another. It might even be doubted whether it is meaningful to ask whether space twists the objects which are contained in it, for such a question presupposes a more ultimate Space with reference to which any distortion must be measured. Contemporary physicists talk about a space which expands and which is wrenched in the proximity of large masses of matter. But such talk has meaning, I think Alexander would insist, only if an Absolute homogeneous Space is presupposed into which a partial space may expand and in relation to which a partial space may be said to twist.

C. The *status of quality* is a crucial issue for any metaphysical system. How, in Alexander's system, is quality related to the stuff of the universe? How is quality related to pattern? What exactly is the metaphysical status of quality in Alexander's universe?

And Alexander finds the answer in terms of Space-Time. Any quality is identical with (or at the least, correlated with) a particular configuration of Space-Time, *i.e.*, a particular motion. Thus, he speaks of the quality "sweet" as a "definite and particular motion" and of the "motion of whiteness (which for us is white)."[35] "Quality," he says unambiguously, "is . . . equivalent to a certain spatio-temporal complex."[36] Quality is not merely superimposed upon or correlated with certain configurations of

[34] I, 209.
[35] I, 274, 275.
[36] II, 312.

Space-Time. Quality *is* Space-Time. And this is so, necessarily, because there is nothing beyond Space-Time.

The problem of the relation of quality to its basis is best typified in the relation of mind to body. Mind (or its equivalent, consciousness) is not merely "correlated" with its neural basis as the physiologists describe that neural basis. "Correlation is . . . an inadequate and misleading word to describe the relation of the mental to the corresponding neural process . . . In truth, according to our conception, they are not two but one."[37] They are thought of as two only because they are known through different modes of apprehension. "That which as experienced from the inside or enjoyed is a conscious process, is as experienced from the outside or contemplated a neural one."[38] This rings very much like a Spinozistic double-aspect theory. Indeed, Alexander has on one occasion made acknowledgments to Spinoza. "If," he wrote not long after the appearance of *Space, Time, and Deity,* "they inscribe on my cinerary urn in the crematorium, Erravit cum Spinoza, I am well content."[39]

But he does not always speak as if it is the mere modes of apprehension which differ. He speaks as if the neural process and the mental quality were actually two different entities. On the same page which contains the previous excerpts, he writes, "It has then to be accepted as an empirical fact that a neural process of a certain level of development possesses the quality of consciousness and is thereby a mental process."[40] This single sentence represents the confusion of the two senses: if the neural process merely possesses the consciousness quality, then it is not identical with that quality; but if it is identical with that quality, then how can it be said legitimately to possess it. Does the quality of consciousness *possess* the quality of consciousness? Is the quality of redness *contained* in the quality of redness? But more of this later.

Alexander's indecisiveness as to how exactly he is to relate quality to Space-Time is amazing. At times, he definitely says,

[37] II, 5.
[38] *Ibid.*
[39] *Mind, op. cit.,* p. 423.
[40] II, 5.

as I quoted him previously, "Quality is equivalent to a certain spatio-temporal complex."[41] At another time, he will speak as if qualities merely were included within a certain spatial groupings and were, so to speak, attached to motions: "Each quality inheres in the substance because it is included in the space which unifies the substance. Thus the proposition, this sugar is sweet, means that the universal sweet in an individualized shape . . . is found within the volume of the sugar."[42] Sometimes he speaks of the quality as "tacked on" to the space: "Thus a substance in respect of its qualities may be described as a space of a certain contour stippled over with qualities."[43] At times he will refer to "the motions at any rate which *correspond* to the qualities;"[44] at others, he will talk of "the motions of whiteness and sweetness,"[45] and at still others he will speak of "the motions to which qualities *belong*."[46]

To the question, then, what is, for Alexander, the relation of quality to Space-Time, the answer seems to be—Who knows?

D. Alexander's position with regard to *universals* is perhaps less variegated although hardly less obscure. In the first place, he is quick to insist, as against the idealists (especially T. H. Green) that "universals of physical things are non-mental,"[47] that "Relations . . . are in no sense subjective or the work of the mind,"[48] and that

> "The laws of the construction of things and those of the relations of things to one another are not therefore inventions of the mind imputed to nature, but part and parcel of the constitution of nature . . . as if thought could make anything real which it does not find."[49]

[41] II, 312.
[42] I, 274, 275.
[43] I, 276.
[44] I, 275, my italics.
[45] *Ibid.*
[46] I, 276, my italics.
[47] I, 220.
[48] I, 246.
[49] I, 227.

Secondly, he tends to speak of universals in terms reminiscent of Peirce and Mead. "A universal," he says, "is a habit."[50] The essence of universality is repetition, and without the possibility of repetition there could be no universality.

The relation of universals to Space-Time is once again not clear. Sometimes he decisively says: "All relation is reducible to spatio-temporal terms,"[51] and, as a consequence, "The universal exists only so far as it is realized in its particulars."[52] The universals do not "come down upon" the world of Space-Time from a supra-temporal and supra-spatial realm, as Plato conceived the process in the *Timaeus*. The Ideas, as Aristotle modified the Platonic interpretation, are intrinsic to and coexistent with the natural world. And it is the Aristotelian modification which Alexander embraces. "Form . . . belongs intrinsically to any finite piece of Space-Time."[53]

But, again, some of his other statements do not permit of so unambiguous an interpretation. "Relations," he says, "are the spatio-temporal connections of things."[54] "Connections"—what are they? Are they no more than spatio-temporal, or is their occupancy of Space-Time only one of their characteristics? "A relation of space or time is a transaction into which the two terms, the points or lines or planes or whatever they may be, enter; and that transaction is itself spatial."[55] It may be spatial—but is it Space?

The question I am driving at is this: Do relations have their being exhausted in terms of Space-Time? A naturalist like Alexander who believes that Space-Time is everything and that there is nothing beyond Space-Time must give an affirmative answer to the question. A good deal of the time, Alexander does give such an answer. But some of the time, he finds himself constrained to give a negative answer, if only by implication. And it is because I feel that it is impossible to avoid giving a

[50] I, 228
[51] I, 239.
[52] I, 222.
[53] II, 49.
[54] I, 249.
[55] I, 166.

negative answer to the question that I criticize him for attempting an affirmative. But more of this in the Critical section, later.

E. And similarly, even *error* Alexander attempts to explain as an entirely naturalistic phenomenon. It is a *natural* phenomenon in the sense that it is common enough and hence occasions no great surprise when it occurs. But for Alexander it is further a *naturalistic* phenomenon in that it is actually grounded in reality. It is Alexander's own responsibility that making it thus real seems to deprive error of its erroneousness.

But at any rate, this is Alexander's interesting, novel, and ingenious manner of solving the question as to the status of error. Error, for him, is nothing more in essence but a "displacement" of natural elements from their natural positions and a "squinting" them into new juxtapositions. It is unnecessary to posit a new shadowy world to act as the habitat of error and illusion: error is merely a misinterpreting of the natural world. "Error does not give us a new and more shadowy being than the spatio-temporal reality, but is the world of determinate being misread."[56]

It is the mind which intrudes into the natural order and rearranges it. The "mind interferes with the world of things and disarranges it."[57] But that is not to say that hence illusions are in some sense mental. Here again G. E. Moore's distinction between mental act and non-mental object which Alexander characterizes as the distinction between "enjoyment" and "contemplation" is of value. Although the cause of the rearrangement which constitutes an error is a mental act, the elements as rearranged are not themselves mental. "Even illusory appearances are non-mental" although "introduced by the mind."[58]

As a result of perspective, we may often see two things just next to each other which in reality are quite distant from each other. This is the sort of thing which Alexander conceives as occurring in error. A final, somewhat lengthier, quotation will serve to show this:

"The object, with which the mind is brought into compresence by virtue of an act initiated by itself, is transferred from its place in the

[56] I, 202.
[57] II, 212.
[58] II, 185, 186.

world into a place to which it does not belong. The illusion is a transposition of materials. . . . Thus I do not make the green which I see in the illusory sensation of hallucination . . . I select it out of the great external whole of Space-Time with all its contained qualities. . . . The mind squints at things and one thing is seen with the characters of something else."[59]

This, then, is Alexander's interpretation of error and of its status in Space-Time. And this completes the expository section of this treatment of Alexander's concept of Space-Time. There is, of course, much in his epochal two volumes which I have not discussed. I have omitted most of the epistemology, of the psychology, of the discussion of categories, *etc.*, *etc.*

But it was not my intention to consider everything in *Space, Time, and Deity*. That would have been next to impossible, anyway. I have aimed merely to include all the important considerations relevant to Alexander's concept of Space-Time. And, trusting that I have accomplished this, I leave exposition to go on to criticism.

II. CRITICISM

A. *Quality*. My first criticism of Alexander has to do with his disposal of qualities. Of course, first there is the question what precisely does Alexander do with quality, for, as we saw, he seems to do a number of different things. He seems at times to identify, outright, quality with Space-Time bits; at others, he seems to think of quality as "tacked" on to sections of Space-Time; and at still others, he seems to interpret quality quite conventionally as something distinct from Space-Time though contained within it.

It is, of course, a moot question which interpretation really represents Alexander's true view—if such there be. Statements asserting all three can be quoted, as I did quote some.

But I shall consider the first view as Alexander's. I shall do so, first, because statements which most unequivocally assert it can most easily, perhaps, be found; secondly, because it is in harmony

[59] II, 214, 215, 216.

with Alexander's "naturalistic" temper, devoted toward resolving everything to Space-Time; and thirdly, because it is the most original and unorthodox of the three views. When I come to discuss universals, later, where, as we saw, the same ambiguity holds, I shall again choose the "naturalistic" view as Alexander's for the same reasons.

What then, is wrong with the view of quality as identical with a portion of Space-Time? Just one thing: it isn't so. Quality is not equivalent to a portion of Space-Time; if it were it would be a gratuitous tautology to say so. It is because the concepts have different intensions that it even conveys any meaning to say that they are the same. It is meaningful to say of two concepts which have different intensions that they have the same extension; but it is tautological to equate two concepts with the same intension. It is because, *a priori*, we (and Alexander) understand two different things by "quality" and "bit of Space-Time" that we grasp the meaning of the statement, "Quality is equivalent to a certain spatio-temporal complex." And we grasp its meaning to see its falsity. The fact that we do understand it proves its falsity.

Alexander finds himself in a dilemma:

(a) If he really reduces quality to Space-Time, he does violence to the empirical facts;

(b) but if he refuses to reduce them, he is not a naturalist, not a monist, Space-Time is not all-sufficient.

And this dilemma runs through all his writing. It is responsible for the waverings and ambiguities upon which we have previously commented. When the monistic naturalistic urge is dominant, he makes statements such as, "Everything is a complex of space-time."[60] And when the empirical strain is dominant, he speaks of *new*, *emergent* qualities in the Space-Time which, presumably, is the same forever,[61] or of stuff as the "*recipient*" of quality.[62]

[60] II, 71.
[61] II, 59.
[62] II, 47.

This whole problem is connected with the question of analysis. The physicists, it is said, analyze all material entities into electrons and protons (or whatever the units are now—the "deutrons," "neutrons," and "positrons" are multiplying with bewildering rapidity). Does the world when so analyzed become "nothing but" electrons and protons, as Alexander's world tends to become "nothing but" Space-Time? The reductionists are quick to make one modification: the universe, they say, is nothing but electrons and protons *in certain configurations*, (and then hedge by insisting that "configurations" i.e. relations i.e. universals are nothing beyond their embodiment in electrons and protons—more of this later).

Now, when electrons are concatenated into new configurations, new qualities are superimposed upon the merely electrical of the primeval units. It is legitimate, I suppose, to describe that fact by saying that the electrons receive new qualities. But I think it is an illegitimate distortion of language to say that what is newly achieved is still nothing but electrons and their properties. A neural process of a certain *Gestalt* blossoms forth into mind and consciousness. But mind is not equivalent to neural fibers and impulses of whatever pattern. Mind is a consequent of, but not identical with, neural patterns.

This is not to disavow that many entities are literally identical with the units and relations into which they may be analyzed. Consider, e.g., a furniture factory turning out chairs and tables and dressers of maple, say. It is quite proper to say of all these objects that they are all composed of the same stuff in different configurations. Analyze a chair into its material and the form of that material, and you have the chair—there is nothing left over. Or describe a triangle into its terms and their relations—"the space enclosed by three straight lines intersecting in a plane"—and you have completely described it.

But try as hard as you will, I insist, you can never get the quality red from a certain disturbance in the ether (if there is such a thing as the ether). They just are not the same. I know it has been fashionable among certain quarters to affirm just this abhorred identity. But no amount of fashion can cancel the

manifest absurdity of the identification. It may be that this matter is one of the ultimates of philosophy, so that its truth is warranted not by any complex chain of argumentation but by direct "intuition." If so, it is unfortunate that the intuitions of some people have led them astray.[63]

I am not raising the question of genesis. It seems to me that often realists think that they have accounted for an entity when they have realistically explained its genesis. It may be that redness can arise only when a wave motion of a certain pattern occurs. But that says nothing of its status. And the status of quality, I insist, is not that of being identical with spatio-temporal complexes.

What then is the status of quality? If I may be permitted a slight digression, I should say that quality is best viewed as one of the two great ultimates of reality—the other being relation. Space-Time is not an ultimate; it is merely one of the most generic of qualities. All that Alexander says of the magnificence and ubiquity of Space-Time applies (more legitimately) to quality and relation. They are universal and ultimate. Sometimes quality and relation correspond (as in color). Sometimes relation exists without quality (as in logical and mathematical relations). Sometimes quality exists without relation (as in deity, if there are deities). But whenever they exist, each is irreducible to the other. There is an infinite number of qualities most of which are independent and irreducible. Green, *e.g.*, is not reducible to yellow and blue. Genesis is not the same as status. Qualities which are reducible are such as: "savory," which synthesizes two modalities of quality and is resolvable into "sweet" (taste modality) and "fragrant" (smell modality); and qualities such as "red-and-hard-and-large," which is resolvable into: "red," "hard," "large," and "juxtaposition-of-red-hard-large." Relations are perhaps more easily reducible one to the other, as *Principia Mathematica* demonstrates. But even here I would be wary of too glib resolu-

[63] The issue has perhaps best been crystallized in recent philosophy in the discussion of the question whether the quality "good" can legitimately be identified with a biological pattern. See R. B. Perry, *General Theory of Value* for an affirmation and G. E. Moore, *Principia Ethica* for a denial.

tion. The universe is rich and diverse; and too great an eagerness for simplification should not blind one to its richness.

Such a system, I think, disposes of the difficulties which Alexander meets when he makes Space-Time fundamental and seeks to find a place for quality. Start with quality as your *primitif*, and no difficulties arise. If one observes the distinction between quality and relation, he does not find himself in the embarrassing situation in which Alexander sometimes finds himself of doubting whether mind is identical with a neural process, *e.g.*, after having asserted that it is not. To paraphrase the quotation from Bishop Butler which G. E. Moore cites on the title page of his *Principia Ethica*: "Every quality is what it is and not another thing"—even when that other thing is another quality or a relation or even Space-Time.

B. *Relation.* With regard to relations, the situation is similar. An unification with Space-Time is impossible because an identity does not exist. Relation is not the same as a spatio-temporal configuration: indeed, such a statement itself presupposes that it is not. Relation is to be defined in terms of Space-Time. All right. But it cannot be in terms just of blank Space-Time: it is a certain pattern of Space-Time. Exactly. A relation is a relation in Space-Time. Or as Alexander explicitly puts it, "Relations are the spatio-temporal *connections* of things."[64] Was there ever more blatant a tautology! Relations are to be reduced to Space-Time. And in order to make the reduction fit, relation is smuggled in under an assumed name (in this case, "connections"); so that the illuminating definition we end up with is "Relation is relation in Space-Time!"

Such a result was hinted at when I was considering the physicist's attempt to reduce everything to electronic terms. He is forced generally, to make the reduction one to "electrons *in certain configurations*." So electrons are not adequate to the world, unless "in certain configurations" means just more electrons. That would be a queer situation: three electrons make a jumble; but four form a *Gestalt*! The feats of modern physics (and philosophy) are truly remarkable!

[64] I, 249.

No, unless one is to mean by the words, "Space-Time" something radically different from what we ordinarily mean by them and from what Alexander means by them, they can not be taken to be identical with relation. Relation cannot be reduced to Space-Time. Relation is a fundamental entity and is not to be reduced to anything.

At times Alexander attempts a less radical unification of relation and Space-Time than this outright identification. At these times, he insists merely that relations are to be found always and only *in* Space-Time (rather than making the two identical). "The universal," he says, "exists only so far as it is realized in its particulars."[65] There is no neutral world in which universals have some sort of ephemeral being, waiting their opportunity to be embodied in the spatio-temporal world and retiring there for an eternity of decent obscurity after their dissolution in the natural world. There is only one world: the spatio-temporal world; and if relations have no existence there, they have no existence.

But though this naturalistic symphony may be less radical than straight identification, it is no less unsatisfactory. Universals, alas, are not always actualized in particulars. The poets have ever bewailed the finite approximations, merely, of the actual to the divine ideal. A universe which has existence only insofar as it has spatio-temporal embodiment is a meager universe indeed.

It has not even a past and a future. The universe which exists at any moment is the universe *as it exists at that moment.* The future, by definition, is that which does not yet exist; the past, again by definition (real definition, not verbal), is that which no longer exists. And a Creator would be required to recreate anew an entire universe at each succeeding moment; and to preserve in some manner the past, so that we could look at it and know it even after it had, apparently, gone. It is not a solution to say that "Our consciousness of past and future is direct,"[66] and that hence in memory, the object of awareness is literally

[65] I, 222.
[66] I, 133.

present to the mind.[67] For how can the consciousness be direct if there is nothing there of which to be conscious!

Past and future, errors and illusions, logical concepts and scientific fictions—all must have some sort of being other than existence. The spatio-temporal realm is too limited in its confines to admit them. But admission they must have into some realm. They are thought of and talked about and are necessary for an intelligible universe. There is need for a realm of subsistence.[68]

C. *Error.* Certainly if a naturalistic interpretation should be ruled out *a priori* for any concept, that concept is error. Error is the most unnatural of unnaturals! It is because it seems so incongruous with the rest of nature that it requires any special treatment at all.

And obviously, Alexander's attempt is unsuccesful. The elements, he says, are real but are misplaced. "Illusions are the real world seen awry or squintingly."[69] What is in the real world in juxtaposition, is in illusion, separated; and what, in the natural world, is separate is, in illusion, together. But, remember this: for a realist relations are as real and as objective as the elements they relate. And elements in any relation must find themselves a place in the natural world (for a naturalist) or wander uncared for in nothingness. If we see certain elements "awry," in relations other than their natural ones, they must, for a naturalist like Alexander, find some existence in the spatio-temporal world or vanish entirely. The fatal point, of course, is that as we relate them in "illusion," they have *no sort of existence in Space-Time at all.* Space-Time is not sufficient to house them. Let A. O. Lovejoy drive home the point with a little tale of an imaginative architect.

"If an architect in Chicago, after dining too well, dreams of a structure having the façade of the Church of the Madeline and the nave of Cologne Cathedral, surmounted by the dome of the Capitol at Washing-

[67] I, 113.
[68] See E. B. Holt, *The New Realism*, pp. 303 ff; E. Spaulding *The New Rationalism.*
[69] II, 216.

ton, culminating in a Chinese pagoda, the physical reality of this picturesque edifice is supposed to be shown by the fact that all four of the parts composing it are physical realities. But they are all in different places, and none of them in Chicago. . . . The parts of the architectural nightmare do not remain in the places, i.e. the positions relative to one another, in which the parts of the physical world with which they are identified are found. In the dream they are, in empirical fact, in immediate juxtaposition to one another."[70]

In spatio-temporal reality, they are not. And hence Space-Time is inadequate. No such fantastic monstrosities exist. Either they "subsist" or they are nothing. This is another of the uses of a realm of subsistence.

And with this I conclude my discussion of Samuel Alexander's concept of Space-Time. I find that I cannot embrace his metaphysical system because of certain inadequacies in that concept. But that does not bar me from expressing my high admiration for his work—for the sober scientific attitude that permeates *Space, Time, and Deity*, for the brilliant insights which adorn its pages, and for the greatness of the man who conceived and executed it. I should not wish anyone to think for a moment that I presume to have disposed of Alexander's concept of Space-Time. It contains much worthy of serious consideration. I echo Broad's words: "... a great work of philosophic speculation, nobly conceived and conscientiously carried through."

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[70] *The Revolt Against Dualism*, pp. 70, 71.

Structure, Function and Growth

BY

LAWRENCE K. FRANK

I

TODAY we are in the midst of a far-reaching shift in scientific thought involving the recasting of many of our long-cherished ideas and preconceptions. To some this appears but the orderly evolution of scientific thought, while to others it portends a revolution in both the ideas and the methods of scientific inquiry.[1]

It is clear from the history of science that a major change in ideas and conceptions usually disposes of a large number of problems and perplexities, not by furnishing any immediate and clear-cut solutions, but by revealing the irrelevance of the questions. That is to say, a new framework of ideas and conceptions, by superseding an older framework in terms of which the current scientific problems have been expressed, renders those problems obsolete and by so much contributes to their rapid dismissal from contemporary thinking. This process has been described in the statement that most of the problems which have occupied

[1] Cf. A. N. Whitehead, "Science and the Modern World;" G. N. Lewis, "Anatomy of Science;" P. W. Bridgman, "Logic of Modern Physics;" William M. Wheeler, "Emergent Evolution," also "Present Tendencies in Biological Theory," Scientific Monthly, February 1929; Ritter and Bailey, "The Organismic Conception," University of California Publications; F. S. C. Northrop, "Science and First Principles;" Ortega y Gasset, "The Modern Theme," pp. 135–152, esp. pp. 139–142.

the attention of man have never been solved; they have merely been forgotten or postponed.[2]

The reformulation of problems and the development of new conceptions are no simple tasks and we shall fumble and distort both the old and the new in our endeavors to achieve them. However difficult, they cannot be evaded nor can they long be postponed.[3] Especially in the field of biology and psychology there is need for more discussion of the changes impending in scientific work, as indicated by the conceptions and related ideas of emergence ("holism"), the "organismic" conception, and the introduction of the idea of relativity into biological thinking. The present situation has recently been described by F. S. C. Northrop in the following statement:

"Living organisms exhibit among other properties an atomic or physico-chemical constitution, and form or structure. These two factors appear in our title as entities and organization. Historically science has never succeeded in finding a theory of first principles which will do justice to both of these attributes. Always it has had to choose between two theories which provided meaning for one at the expense of the other. This is the crux of the issue between mechanism and teleology and between atomism and organicism. In history it appears in the conflict between the Aristotelian and the modern conception of science. Aristotle, approaching living creatures through description and classification by way of natural history was impressed by their form and organization, and hence introduced formal as well as material causes. The result, as he saw, and accepted in his metaphysics, was the rejection of physical atomic categories. Matter became, along with form, the mere attribute of a dynamic teleological activity. Modern science, on the other hand, approaching the living creature through the experimental investigations of Harvey, Lavoisier, Liebig, Wöhler, and others found

[2] "The history of science is a record, not so much of the progressive discovery of truth, but of our gradual emancipation from error." President Kemp Smith, Edinburgh University, Inaugural Address.

[3] Cf. J. H. Woodger, "The Concept of Organism and the Relation Between Embryology and Genetics." Quarterly Review of Biology, Vol. V, No. 1, March 1930.

Cf. also emphasis upon need of "operational concepts" in "Logic of Modern Physics," by P. W. Bridgman.

it necessary to accept the physico-chemical theory of both living and non-living systems. The result, as Claude Bernard saw and as J. S Haldane has emphasized more recently, is the failure to account for organization. Ancient biology provided for form with a resultant rigid fixity of types at the expense of entities and the fluidity of species; modern biological theory provides for the non-fixity of types and the presence of physico-chemical and genetical entities at the expense of organization."[4]

The growing realization of the significance of time for biological studies and the consideration of the problem of growth are further evidence of what is about to take place in biological thinking.

II

If we reflect upon the conception of a space-time manifold, we are immediately faced with certain implications that may be briefly stated as follows: Every existent (such as an organism) may be approached as a space-time configuration, subject, within the limits of instrumentation, to measurement. While we may employ the measures of extension and mass to ascertain the spatial dimensions of an existent and the measures of time sequence and of duration to ascertain the temporal dimensions of an existent, a rigorous application of the conception of space-time will preclude any separation of the spatial from the temporal dimensions.

Dr. Carrel has expressed this conception in a recent paper as follows:

"During development, an animal extends simultaneously in time and in space. Temporal extension is absolutely indispensable to spatial extension. Growth is unthinkable except along one temporal and three spatial axes. In this manner, time is given a spatial representation and considered as a fourth dimension. But such a convenient assumption is probably not accurate. In fact, each spatial dimension also contains temporal elements. At every point of the living things created in space

[4] "Entities and Organization in Current Biological Theory," by F. S. C. Northrop. Sigma Xi Quarterly, Vol. XXI, No. 1, March 1933.

by time, space and time are indissolubly united. Biologists should have conceived long before Einstein and Minkowsky that space and time are not separate entities but constituent elements of a four-dimensional continuum."[5]

Concretely, the foregoing means that the problem of structure, organization, or form is the problem of spatial configuration or sequence, while the problem of function, process, or activity is the problem of temporal duration or sequence. Structure and function, however, are not separate factors or entities, but two different aspects of, or observations upon, a space-time configuration or organism. There can be no rigid dichotomy of structure and function, of organization and process, of form and activity, *because a structure is a persistent function, while a function is a series of changing structures.*[6]

A function or process is a series of changing spatial configurations, each one of which is an event in the temporal series we call functioning. As we are learning to use instruments for *temporal* magnification, e.g., the "slow motion picture" and the stroboscope, we are discovering that a function or process occurs through, or as, a series of structural deformations or spatial alterations: We see a succession of spatial configurations, each merging into the next, as the structure is deformed; these deformations or spatial alterations are the function. "Tissue and blood cells are always in process of becoming. They do not show their true physiognomy when they are examined under the microscope. Cinematography alone is capable of recording their fourth dimension. Fixed cells appear on the film as mobile as flame. Their surface is never smooth. In some places it bubbles like boiling water."[7] We may measure these structural alterations spatially and measure the functional activity temporally

[5] "Physiological Time," by Dr. Alexis Carrel. Science, Vol. 74, No. 1929, p. 620.

[6] Cf. the writer's earlier statement of this view in "The Development of Science," Journal of Philosophy, Vol. XXI, No. 1, January 1924.

See also his "Problem of Learning," Psychological Review, Vol. 33, No. 5, September 1926.

[7] "The New Cytology," Dr. Alexis Carrel. Science, Vol. 73, No. 1890, p. 300, March 20, 1931.

but these measurements are but different aspects of the unified space-time configuration, as Dr. Carrel has stated in the following:

"Secondly, structure and function are two aspects of the same thing. One can not consider them separately. Each structural detail possesses its functional expression. It is through the physiological aptitudes of their anatomical parts that the life of the higher animals is rendered possible. Likewise, the life of a community of ants depends on the physiological aptitudes of the individuals of which it is composed. When cells are considered only as structural elements, they are deprived of all the properties that make them capable of organizing as a living whole. Within the organism, they are associated according to certain laws. Cell sociology results from properties specific to each cell type. Among these properties, some manifest themselves under ordinary conditions of life, while others remain hidden. Tissues are endowed with potentialities far greater than those which are apparent. But these potentialities become actualized only when certain modifications of the internal environment occur, as, for instance, when pathogenic agencies are at work within the body. The significance of a given structural state is bound to the knowledge of the corresponding physiological state. Structure and function must be considered simultaneously."[8]

A recent paper by G. E. Coghill discusses this conception:

"According to current thought, nothing is static; everything exists not only in a 'frame' of space but also in a pattern of time. It becomes, then, a question not of matter *and* motion, but of matter *in* motion. Neither is it a question of structure *and* function; but it is a condition of structure *in* function; for all structure is in action, and the action of a structure is its function. Argument as to whether structure precedes function or function precedes structure, or in other terms, whether structure *causes* function or function *causes* structure is beside the mark, for neither can exist without the other at any point in space or time: they merge, in fact, into a space-time relation. Accordingly, if we think structure as static, we can attain in science only description that is infiltrated with metaphysics in the form of an anthropomorphic and mystical conception of cause and effect; but if we think structure *in* function as a space-time relation, as we must in a purely scientific discipline, we may

[8] "The New Cytology," by Dr. Alexis Carrel. Science, Vol. 73, No. 1890, p. 298, March 20, 1931.

hope to attain to all the understanding of behavior that science has to offer."[9]

Concomitantly with the spatial alterations of an organism, energy transformations occur and are exhibited as light, heat, chemical and electrical activity, and changing mass. These data are also used to study function and process, but since they are so dissimilar to the structures releasing them, students of functional activity have usually ignored the spatial alterations or deformations that give rise to these energy transformations. It is evident, however, that the sequence of changing spatial configurations, the time sequence or duration, and the energy emitted or absorbed, are but three different ways in which the space-time configuration or organism is revealed. An organism exists as a space-time configuration which functions as a series of spatial deformations occurring in sequence for a period of time, accompanied by energy transformations. These alterations in the space-time configuration are the energy transformations which are propagated and recorded in various ways.

It is evident that spatial measurements of structure must be made at some stage of functional activity, that is, at some phase of the spatial deformations which the organism undergoes as it functions. There are several possible choices of such time: at the moment of minimal spatial deformation or functional activity (at rest); at the upper and lower limits of spatial deformation from which an average may be calculated; or after death, when functional activity has ceased and further alteration (through fixatives) has been arrested. From these practices it would appear that we apply the term structure or organization to one of the many spatial configurations exhibited by the functioning organism. This means that we arbitrarily select the set of spatial dimensions for which the term structure is used and that, strictly speaking, there is no structure to an organism, in the older sense of a definite, static, spatial arrangement with precise dimensions, as Coghill has emphasized (supra). Rather

[9] "The Neuro-Embryologic Study of Behavior: Principles, Perspective and Aim," by Professor G. E. Coghill. Science, Vol. 78, No. 2016, August 18, 1933, p. 137.

we have spatial configurations which recur or persist most frequently and those are the structure which exists through continuous functioning. Thus a function is to be regarded as the sequence of structural alterations that occur in and with energy transformations for definite periods of time.

Organic configurations or structures cannot exist without functioning, i.e., undergoing transformations, nor can functions occur without alterations in the spatial configurations through which these transformations take place. Dr. Carrel may again be quoted for his cogent expression of this point:

"The living organism undergoes two classes of changes: rhythmical and reversible, or progressive and irreversible. These changes are as indispensable a part of the body as the tissues and organs described by the anatomists. Anatomy, to use a definition of Woodger, studies the organism in timeless space. Such a study is imposed by methodological necessity. Its object is not the concrete body, but only an artefact. An organism deprived of duration is just as unthinkable as if deprived of spatial extension. Dead organs and histological sections are nothing but useful abstractions. The body really consists of a flux of structural and functional processes, that is, of an uninterrupted modification of tissues, humors and consciousness. Such is physiological duration."[10]

It appears then that structures may be relatively stable with small energy transformations and spatial alterations, or highly unstable so that the spatial dimensions are subject to rapidly alternating deformations with a high rate of energy transformations as shown by absorption and emission of energy. This contrast is to be seen in bony structures of fairly stable configuration and limited energy transformations and in muscular or glandular structures with rapidly shifting spatial configurations and large energy transformations. Even more pronounced is the contrast with liquid or colloidal structures (gels and sals), where the spatial configurations alter with extraordinary rapidity and enormously powerful energy transformations. (Cf. later discussion of fields and quotation from Jerome Alexander, p. 232).

[10] "Physiological Time," by Dr. Alexis Carrel. Science, Vol. 74, No. 1929, December 18, 1931, p. 619.

If the structures of an organism are classified into a hierarchy, according to the instability of their spatial dimensions, they will be found to be arranged in order of magnitude of energy transformations and functional activity because of this relationship.

Furthermore, the greater the functional activity per unit of time, the shorter will be the duration of the spatial configuration or structure from which the energy transformation arises, since, as just indicated, the persistence of a spatial configuration precludes intense functional activity or energy transformations; again, the contrast between slowly changing mineralized structure of a bone and the short-lived blood-forming cells within the bone.

The functional activity of a structure involves energy transformations, in the form of absorption and emission of energy (as heat, et cetera) thereby determining the character (i.e., degree of structural elasticity) of the immediately adjacent structures through which those energy transformations may be propagated: the more labile the structure, the more intense is its functional activity and the greater its concomitant energy transformation, and the greater the necessity for contact with a structure of even greater lability and functional activity. Thus we see how solid, liquid and gaseous structures relate themselves in an organism to provide for the interchange or transmission of energy between the organism and its several component spatial configurations and the surrounding medium, liquid or gas.[11]

The theoretical basis for this situation is indicated in a statement by Joel H. Hildebrand:

"I shall now attempt to lay a basis for understanding such a phenomenon. The physical differences between solids, liquids and gases are familiar to all. The kinetic picture of these differences can be stated in very elementary terms. In the solid, the molecules are held in a rigid lattice, each oscillating about its mean position. As the temperature rises, this oscillation increases until, at the melting point, the molecules no longer return to their former domains and the lattice breaks down. From this point on the molecular motion is no longer chiefly oscillatory

[11] See J. Needham, "Protein Metabolism and Organic Evolution," Science Progress, April 1929, for an excellent illustration of this point.

in character, but the molecules are free to wander about the liquid, confined within the surface by the inter-molecular attractions which are so strong that only an occasional molecule is bumped so hard by its neighbors as to be able to escape into the vapor state, where it moves between collisions with other molecules in rectilinear paths, which may be hundreds or even thousands of times greater than its own diameter. I will show this molecular agitation in the liquid state by a motion picture of the Brownian movement. It is evident that the solid state, in which the molecules are oscillating about fixed positions, and the dilute gas, in which they are moving most of the time in straight lines, present simpler theoretical pictures than the liquid state, in which their motions are far more irregular. It is not strange, therefore, that our knowledge of this state has lagged so far behind that of the other two."[12]

It becomes evident then that the growth of specific structural components and their organization into functioning configurations proceed from the space-time sequences and energy transformations involved, thus giving a clue to what underlies Child's conception of the physiological gradient: the proliferation of any structure involves not only spatial configurations but energy transformation of graduated nature which set up space-time sequences, both before and after the proliferation; at the growing end the spatial configuration will be most labile, therefore most active in energy interchange; immediately surrounding it must be a more labile medium for that energy interchange while a less labile structure will arise behind them to give support.

The regeneration of nerve tissue, as Detwiler and others have shown, takes place by growth toward the point of future reunion; this is further evidence for the view that functional activity may be defined as structural deformations, in this case taking place in a medium that makes possible a permanent alteration of the spatial configuration and restoration of the previous dimensions. Thus it will be seen how this conception of space-time sequences, instead of the older concepts of discrete structure and function, enables us to approach some of the more puzzling features of biological problems. By recognizing that growth is not merely

[12] "The Liquid State," by Professor Joel H. Hildebrand. Science, Vol. 80, No. 2067, August 10, 1934, p. 126.

structural change, but is a series of structural-functional alterations, or more precisely, a sequence of deformations of a space-time configuration that cumulate or persist until the original dimensions are shifted, we may escape the difficulty of our previous formulations and advance our problem.

The problem of organic growth or development may be stated as the measurement of the changing dimensions of structure-function activity of the organism, and the discovery of the sequence in which those changes occur. The study of organic development involves therefore the simultaneous measurement of the organisms' structures, functions, and activities, using all available scientific techniques and instruments for the purpose and collecting these data from individual organisms over a period of time sufficient to reveal the developmental process of the individual organism. This is a relatively novel problem to those who have been trained exclusively in the analytic procedures of studying the relation between two variables as revealed in data collected from a large number of subjects at a given instant of time. The attempts to synthesize growth curves by adding statistical data from organisms of successive age groups have not been successful, as the New Orleans symposium on development indicates:

"In the realm of physical anthropology emphasis centered very largely around the question of growth. Papers and discussions . . . were focussed to an unprecedented degree on the individual, as contrasted with the group. Indeed, the sessions devoted to this subject left a rather definite impression that we may be at the beginning of a new era, in which *individual, rather than mass studies, will be foremost.* The importance of observing the same individual over the largest possible period was emphasized by nearly every speaker who dealt with any aspect of development."[13]

Embryology and child development research necessitate the consecutive or longitudinal study of organic growth in a single organism so that these simultaneous alterations in spatial and

[13] Report of A.A.A.S. meeting, New Orleans, December 1931, Section H. Anthropology.

temporal dimensions, and in energy transformations may be measured as they occur in that individual organism, thereby revealing the accelerations and retardations in those different growth processes and the resolution of those discrepancies within the elastic framework of the growing organism. Growth may be regarded as the changes in magnitude, while development is the patterning of those changes and their interrelationships within the changing organic configuration.

It begins to appear, then, that a spatial configuration may be deformed and then return to its previous dimensions as in ordinary functioning; if, however, after such a functional deformation it does not return to its former dimensions, it will begin to enlarge or diminish, with a concomitant alteration in functional activity, as we see in the growth and differentiation of an organism. This conception of growth as a functional process which involves persistent structural changes becomes clear in the light of the preceding discussion of the nature of function and structure as two measurements of the same space-time configuration.

This point of view likewise suggests an altered conception of pathological change, viewed as the end condition of an organism (or its component) where a structure is first deformed beyond its usual limits as it functions under adverse or novel conditions; with each repetition of such extraordinary functioning the limits of so-called normal deformation (functioning) are changed or shifted until the configuration becomes persistently altered to function in the new manner. Dr. John J. Abel has said:

"Disease in its reversible early stages can not be more clearly defined until we are better informed in respect to the many chemical and physical processes and the composition and function of the many microphysical structures that are concerned with the maintenance of the ever-varying 'normal state' of cells or cell states."[14]

Correction of the adverse conditions may lead to a return to normal or substitute structure-function,[15] provided the adjacent

[14] "On Poisons and Disease and Some Experiments with the Toxin of the Bacillus Tetani," by Dr. John J. Abel. Science, Vol. 79, No. 2039, January 26, 1934, p. 70.

[15] Cf. Wm. DeB. MacNider, "The Morpological Basis for Certain Tissue Resistance." Science, Vol. 73, No. 1882, p. 103 et seq., Jan. 23, 1931.

structures and surrounding media will again permit the shift to the old limits of structural deformation. The traditional controversy over "organic" versus "functional" may be resolved by viewing the pathological or disease situation as arising from two different processes: where the abnormal functional activity takes place without persistent structural deformations (pathological changes) we have the so-called functional disease that does not reveal itself in the usual histological picture as it does when the abnormal functioning involves permanent structural changes in the organic configuration. This difference points to the probability of individual variations in the elasticity of structural alterations whereby unusual or abnormal functioning (structural deformations with energy transformation) can occur in some organisms and at some times without involving persistent structural changes.

III

This leads us to the examination of another conception, which we may, borrowing from physics, term the "field," meaning thereby that every space-time configuration, by reason of the spatial deformations and the energy transformations it undergoes, has a definite range of interaction with other space-time configurations. To illustrate, the deformation of, and the heat emitted by, a muscle segment will extend and the heat will be absorbed within certain limits; every configuration within these limits may be considered as lying within the "field" of that muscle segment. This appears in Child's minimal distance of separation for gradients. Obviously, in many organisms the "fields" of interaction have been enormously extended through such means of diffusion or conduction as the nerves, the blood, and the lymph, the latter carrying nutritive substances which act as special "fields" for structural change. "This has led to the discovery of strains of fibroblasts, which, although morphologically identical, differ in their nutritional properties. The food requirements of a given cell type are as fundamental a characteristic as its morphological aspect. It appears that each cell type demands a specific diet."[16]

[16] "The New Cytology," by Dr. Alexis Carrel; supra.

Whenever a space-time configuration enters the "field" of another space-time configuration, the previous limits of deformation, energy transformations, and of duration of both configurations are altered so that the structures and functions of both configurations are changed while such contact persists. Moreover, whenever a space-time configuration is brought permanently into the "field" of another space-time configuration, these alterations will become persistent in the sense that the contiguous "fields" of both will interact, thereby giving rise to another space-time configuration, which will not be the exact sum of the two formerly separate configurations, but rather a different configuration embracing some of the structural and functional characteristics of both. This statement offers another way of approaching the subject of "emergents" and a method of introducing the general idea of relativity into biological thinking, as the later discussion of time will further explain. Obviously the dimensions of a space-time configuration (e.g., the structure and functions of a molecule or cell) will be governed by the "fields" in which it appears, so that the same kind of molecule or cell will have a different structure-function for each "field" or larger configuration of "fields" in which it is comprehended. When we study the molecule in isolation from diverse "fields" or remove the cell from its organic "fields," as in dissection or other analytic procedures, it exhibits only those structure-function aspects which arise from their basic chemical characteristics or the artificial "fields" in which they are studied and fail to reveal their organic characteristics. Moreover, the aggregate "fields" of an organism operate to conserve its organic unity and stability, since all energy transformations within the organism are immediately transmitted to and received by the other "fields."[17]

[17] "L. J. Henderson, Interpreting Willard Gibbs' Phase-Rule, describes similar characteristics of reality when he writes: 'I. Other things being equal, the stability of a system increases with the number of phases, and also with the number of restrictions upon the intensities of energy, e.g., temperature, and upon the concentrations. Thus a system of three phases is more stable than a similar system of two phases; a system of constant temperature is more stable than a similar system in which the temperature is variable; and a system in which the tension of carbon dioxide is constant is more stable than one in which this is a variable quantity. II. Other things being equal, the stability of a system

What we mean by an organism, or organization, is essentially an aggregate of space-time configurations, the "fields" of which interact directly or through media of conduction, such as nervous or vascular tissue. This conception of organization has a wide application, since under it we may subsume the purely inorganic and organic organizations and also the social organizations wherein each participating organism has its behavior patterned by the behavior or "fields" of the other participating organisms. By such an approach to the question of organization as the aggregate of interacting "fields" (interacting activities of the participating entities), we may dispense with the rather mystical notions of organization and organizers so frequently invoked, especially by biologists and social scientists.[18] We may also eliminate the notion of the "whole" dominating the parts since the interacting "fields" of the constituent parts are the whole. Organization and past experience are to be viewed as the patterning of functions or behavior, modifications in the structure-functions of the participating entities that have persisted.[19]

Accepting the implications of the foregoing would eliminate the simple notion of conduction of impulses, as often viewed in nervous tissue, in favor of the notion of space-time alterations of the whole configuration, thus bringing the problem of nervous impulse in line with the problem of light transmission and other energy transformations, wherein a wave of energy transformations passes along or through a sequence of space-time configurations with interacting "fields;" as each point-event in that

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diminishes with increase of the number of its undecomposed constituent molecular species, and the number of different forms of energy, e.g., heat, pressure, electrical potential, surface-tension, which are involved in its activities. (Cf. "The Order of Nature," Cambridge, 1917, pp. 136–7.)'" From "Crossing 'The Rubicon Between Mechanism and Life,'" by Robert Morris Ogden. The Journal of Philosophy, Vol. XXII, No. 11, May 21, 1925, p. 287.

Cf. also "Organization for Physiological Homeostasis," W. B. Cannon. Physiological Review, Vol. 9, No. 3, July 1929.

[18] Cf. F. H. Pike, "Vitalism, Mechanism and Organicism," Science, Vol. 76, No. 1974, p. 384.

[19] Cf. the writer's paper, "The Locus of Experience," The Journal of Philosophy, Vol. XX, No. 12, June 7, 1923.

sequence responds to its adjacent point-events, a wave of energy transformations occurs, but there is nothing conducted in the sense of something put in at the beginning and received at the end. Rather a change of state is initiated at one end of the sequence and is propagated through the interacting "fields," each of which exhibits a space-time alteration and energy transformation in accordance with the pattern of its structure-function. A crude illustration of the conception is to be seen in a string of beads which are of different sizes, a row of small beads, then a row of large beads, then smaller and larger. If the beads at one end receive an impulse it will spread along the whole string and, as it passes, the activity of each segment will be characteristic to the size of the beads. Nothing is moved or transported from one end of the string to the other, but there is a sequence of space-time alterations as each bead (point-event) receives the impulse of its neighbor and in turn sets the next adjacent bead in motion. One segment with small beads will exhibit small spatial deformations, but rapid transmission from bead to bead, while another segment with large beads will exhibit larger spatial deformations and slower transmission. Accompanying these spatial deformations (changes of state) energy transformations occur, as measured in heat, chemical and electrical activity. These energy transformations are the "fields," through which the sequence of configurations interact. When the "fields" of dissimilar configurations are interacting, energy transformations peculiar to that situation occur, as for example, the production of acetylcholine (Dale) and sympathin (Cannon), where nerve and muscle meet.[20]

"A nerve fiber is surrounded by a plasma membrane which, when in the resting state, is polarized with an excess of positive ions on the outside and of negative ones on the inside. The nerve impulse renders the membrane temporarily permeable and depolarized. Positive ions in front of this permeable portion of the membrane stream backward

[20] See Henry Dale, "Chemical Ideas in Medicine and Biology," Science, October 19, 1934, Vol. 80, No. 2077.

Cf. also "Physical and Chemical Changes in Nerve during Activity," Occasional Publications No. 2 A.A.A.S. Supplement to Science, Vol. 79.

through it and then forward to neutralize the negative ions on the inner side. This adjacent part of the membrane thus becomes depolarized, and, according to the theory, depolarization for some reason makes it permeable, and the positive ions beyond it can stream backward through it to depolarize in turn the next stretch of the fiber. In this way a wave of electrical and chemical change spreads along the fiber. This is the nerve impulse."[21]

This conception is of the greatest significance for the problems of biology, neurology, and psychology, where we are wrestling with such questions as the transmission of nervous impulses and similar problems of conduction. According to the conception here set forth, the stimulus at one end, e.g., light impinging upon a sense organ of vision, initiates a series of energy transformations which proceed as a wave transmitted along the sequence of space-time configurations of the organism. In nervous tissue there is rapid transmission, small deformation, and little energy transformation (so we say nervous tissue merely conducts), but as the wave of energy transformations reaches a muscle spindle or aggregate of muscle spindles, energy transformations occur at the point of juncture and are then propagated with relatively enormous deformations, slower transmission and large energy transformations.

Two interesting consequences follow from the above. First, we may eliminate the dichotomy of nervous tissue-muscle spindle and the resulting problem of how one causes the other to contract, for we see the nerve-muscle sequence as one configuration with characteristically different deformations and energy transformations through which we may observe the passage of a wave of changes of state. Our difficulty arises from splitting an essentially unified configuration into two parts and thereby artificially creating a problem of how to get them together again.[22]

[21] "Cutaneous Sensation," by Professor S. W. Ranson. Science, Vol. 78, No. 2027, November 3, 1933, p. 396.

[22] Cf. E. A. Bott, "Criticism and Ways of Inquiry," Journal of Philosophy, Vol. XX, No. 10, May 10, 1923, where he describes this procedure as the "split-field method."

See also the writer's paper, "Causation: An Episode in the History of Thought," Journal of Philosophy, Vol. XXXI, No. 16, August 2, 1934.

If we regard the organism as a single configuration of structure-functions having interacting "fields," then we see that the functional activity of any portion, segment, or organ thereof is the spatial deformation and energy transformation characteristic to that segment or organ. Glands function and secrete, muscles function and contract, nerves function and conduct, and so on, each with its appropriate spatial deformations and energy transformation of heat, chemical and electrical reactions, and the like, as an impulse arising in one segment is transmitted through channels of conduction, or, more precisely, through interacting "fields," in each of which appears the structural deformations appropriate to the magnitude and pattern of its structure-function. A relatively small deformation in a sense organ is propagated through a nerve with great rapidity and small deformation to a muscle or gland where the deformation is large and the accompanying energy transformations are considerable.

"It would thus appear that in the case of living organisms, special structural substances are required for each single form and function, in contrast to artificial machines, which, serving the most diverse purposes, may be constructed from a limited number of materials."[23]

This difference between machines and organisms was described from another angle by Sigaud, the French physician, who pointed out that a machine continues to operate only so long as its dimensions remain the same (i.e., its structure is unaltered), while an organism can function only by undergoing continual alteration of structure (i.e., by deformations and replacements).

The second consequence of this view is that the transmission of the wave of deformations and their accompanying energy transformations are conditioned by the readiness of every portion of the sequence involved to be deformed. That is to say, the muscle fibers must be in a state of readiness to be activated, with an adequate supply of energy to be transformed, or the total reaction will not occur or will occur in lessened magnitude. This

[23] "Individual Differences in Human Blood," by Karl Landsteiner. Science, Vol. 78, No. 1894, April 17, 1931.

implies that the irritability of the eye to light is conditioned by the readiness of the muscles to react or, in other words, that the responding organs evoke the stimulus by their readiness to react, which, in the sense organ end of the configuration, appears as a condition of heightened irritability. Thus, readiness to respond generates and patterns the stimulus as much as the stimulus evokes the response.[24] What we call instinct, then, is not a preformed reflex arc specific to a particular function nor a highly differentiated neuromuscular pattern reserved solely for instinctive behavior, but rather a condition of heightened irritability and readiness to respond, generated at specific times and places by the growing organism and touched off by the appropriate situation-event.[25] The various organ systems and segments of an organism, being within the "fields" of each other are thereby maintained in a state of readiness to react (i.e., change structure and thereby function) in response to activities of other segments. Thus, the stimulus to function is in a real sense created by the responding organism as we see in sexual maturation that sensitizes the organism to the potential mate.

Since this newer formulation serves to restate our problems in a more serviceable fashion, it is urged here as a desirable way of reconsidering the situation. Succinctly, it means that if the muscle or gland were not prepared to respond, the eye would not see the stimulus. Stated positively, this means that the eye sees only what the organism is prepared to respond to, which is another way of stating the basic principles of the Gestalt School in Germany, with its emphasis upon the selective, patterned perception of the organism.[26] An even more telling example of this principle is seen in the Rorschach test where the individual sees in the "ink blots" what his organic experience has specifically compelled him to see.

[24] Cf. L. L. Thurstone—"The Stimulus and Response Fallacy," Psychological Review, Vol. 30, No. 5, September 1922.

[25] Cf. the writer's paper, "The Problem of Learning;" supra.

[26] Cf. Wolfgang Köhler—"Gestalt Psychology;" also see the writer's paper, "The Problem of Learning," supra, where this interaction of organism and situation is discussed at length.

K. S. Lashley has stressed this point in discussing cerebral functions:

"There is evidence that the activity of every nervous center is conditioned by a variety of factors. Its general level of excitability varies and a low threshold or state of tonic activity is maintained by excitation from many sources. Steadiness and continuity of discharge are likewise maintained by agencies other than those which are concerned in the specific patterning of reactions. In addition, there are many indications of a preliminary integration or setting of nervous mechanisms which may then maintain a potential organization until activated by excitation from other sources. Thus it is possible, by brief stimulation of a motor point in the cortex, to modify the motor responses elicited on stimulation of distant points and such an altered excitability or motor set may persist for an hour or more before it gives way to the original pattern of response.

"These processes, which we may term priming, steadying and preparatory adjustment, seem to be subthreshold for overt activity: some sort of partial activation or tonic excitation of centers. In addition to these, there are activating mechanisms which perhaps impose additional patterns of integration or perhaps only raise the tonic excitation above the threshold for motor expression.

"In the motor field we have evidence that many structures participate in activity without actually determining the specific pattern of skilled movement."[27]

IV

Anyone who will pursue the implications of this view into biology, medicine, psychology, and the social sciences, will begin to see what a revolution impends in our thinking as these newer conceptions are brought into those fields. Older formulations and problems and the scientific work to which they give rise, are rendered obsolescent as we catch a glimpse of the new problems generated by these altered approaches.

The problem of evolutionary development and of learning receive a new significance when we employ the conception of "fields." In so far as we envisage the interaction of space-time

[27] "Mass Action in Cerebral Function," by Professor K. S. Lashley. Science, Vol. 73, No. 1888, March 6, 1931, p. 253.

configurations (organisms) and the "fields" of the environing situation, we may contemplate the "emergence" of new structure-functions. The organic configuration will undergo permanent alteration through the impact of new "fields," arising from large-scale climatic and geological changes, or shifts to a different medium, as for example, in the change from a marine to a littoral environment involving contact with the "fields" of the atmosphere in addition to the "fields" of the water.[28] We thus may envisage a hierarchy of space-time configurations with an increasing irritability or susceptibility to the "fields" of other space-time configurations. Physics is concerned with the interaction of like "fields," chemistry with the elementary complex of diverse "fields," while biology (including psychology and the social sciences) studies the interaction of diverse "fields" within and without the organism, including learned behavior and culture. The more complex an organism, the more "fields" of different kinds it comprehends within the space-time configuration we call its structure and function, and likewise the greater the variety of external "fields," e.g., stimuli, to which it can respond.

It is evident that the conception of cause and effect, which physics has already relinquished for the most part, must also be eliminated in the biological sciences. Instead of seeking so-called causes in organic life and behavior, we must learn to think of events taking place within an organic configuration with a past history so that organic development and functioning, breakdowns, etc., are to be viewed as different ways in which the organic configuration reveals its make-up, the on-going energy transformations, and the realization of its unique life career. Thus, biological thinking may begin to assimilate the more recent transformations in thought initiated in physics and astronomy and certain to influence our whole climate of opinion.

The so-called "cause" may be defined as "any event which delivers energy to another event, in distinction from a limita-

[28] Cf. the discussions by F. H. Pike of the crucial character of the shift from water to air breathing; also Needham's paper (supra); also the writer's paper, "The Problem of Learning," (supra); also F. S. C. Northrop, "Science and First Principles," Chapter V, p. 224 et seq.

tion,"[29] thereby releasing or initiating energy transformations in the organism that exhibits the so-called "effect." But this "effect" is not the product of the "cause"; it is the alteration in structure-function of the organism touched off by the energy of that "cause." To understand the "effect" we must study not merely the antecedent "cause" or energy impact, but the organism, its structures-functions, prior history and present state. In such experiments as artificial fertilization, X-ray treatments, and in conditioned reflexes (Pavlov), we are beginning to see how to evoke given "effects" by use of equivalent forms of energy and the conditioning of the organism to respond to surrogates for the original stimulus. It is evident that the stimulus (original or surrogate) touches off a response, the nature and extent of which is a function not so much of the stimulus, as of the prior history and present state of the organism. Thus it is becoming evident that the same stimulus or event will elicit different responses according to the condition or state of the responding organism and the structure-function of the specific organs upon which the stimulus-substance impinges. Accordingly, many of the diverse properties of endocrine secretions may be indicative of varied responses of different structure-functions to the same secretion that thereby appears to have different properties because of the diverse organs or structures it stimulates (cf. the pituitary).

One further consequence of these views is that any given organism (space-time configuration) may yield a large number of data which record or measure the spatial and temporal dimensions and the energy transformations accompanying the changes in structure-function. Therefore, to take a set of spatially-measured data and a set of temporally-measured data or data of energy transformations from the same set of organisms and then attempt to demonstrate that one is the "cause" or the "invariable antecedent" or the "determinant," et cetera, of the

[29] Chappell, M. M. and Pike, F. H., "The Organic World and the Causal Principle: a Criticism." Science, Vol. LXXII, No. 1869, Oct. 24, 1930, p. 427.

See also the writer's paper, "Causation: An Episode in the History of Thought;" supra.

other, is to ignore the foregoing implications of the conception of a four-dimensional time-space manifold. Nevertheless, it is suggested that a large part of contemporary scientific work, particularly in biology, psychology, and the social sciences, consists in just this artificial dichotomization of two or more sets of data derived from the same organisms, with a triumphant putting them back together again with the aid of more or less elaborate statistical techniques, a method already described by E. A. Bott as the "split-field" method. The contrast to this analytic procedure has been cogently expressed by Dr. Alexis Carrel:

"The success of the new method in bringing about the discovery of so many phenomena must be attributed to its power, which histology, physics and chemistry lack, to apprehend the complex system formed by the tissues and their environment. The concepts and methods of physics and chemistry are adapted to the atomic and molecular levels of the organization of matter. When applied to the cellular and supracellular levels, they detect only phenomena of the atomic and molecular orders. On the other hand, cytology and histology are concerned exclusively with the form of cellular and supracellular organisms. Therefore none of these sciences alone is capable of dealing with physiological phenomena, such as organization and adaptation, which belong to the supracellular order and are the expression of sociological laws. The laws specific of physiology, said Claude Bernard, are the laws of organization. Such are precisely the phenomena and the laws that the new cytology endeavors to discover by coördinating, through its own techniques, the data supplied about cells, tissues and organic fluids by physics, physical chemistry, chemistry and classical cytology and histology. Studied in this manner, cells and tissues appear as being endowed with properties which make them not only the building stones, but also the builders of an organism capable of developing, maturing, growing old, repairing wounds and resisting or succumbing to diseases. It is with such an aspect of the tissues that embryology and pathology, as well as cytology, should be concerned.

"Experimental embryology, which searches for the mechanisms of the determination and differentiation of structures, may never completely fulfil its purpose unless its conception of cells and tissues, and its techniques, are modified."[30]

[30] "The New Cytology," by Dr. Alexis Carrel; supra.

A forecast of these more recent developments, which was made in 1885, is worth quoting here:

"Dr. Haldane holds that to the enlightened biologist a living organism does not present a problem for analysis; it is, *qua* organism, axiomatic. Its essential attributes are axiomatic; heredity, for example, is for biology not a problem but an axiom. 'The problem of physiology is not to obtain piecemeal physico-explanations of physiological processes' (I quote from the 1885 address), 'but to discover by observation and experiment the relatedness to one another of all the details of structure and activity in each organism as expressions of its nature as one organism. . . . That a meeting-point between biology and physical science may at some time be found there is no reason for doubting. But we may confidently predict that if that meeting-point is found, and one of the two sciences is swallowed up, that one will not be biology.' "[31]

We should also note here the need for a more critical use of the concept of time and of temporal data, and for a recognition of the relativity of time for each organism.[32] Different organisms live at totally different rates: a year or a day are not the same for different energy complexes which may move or function at different velocities, depending upon their unique structure-function. To quote from Jerome Alexander:

"A most striking example of optimum dispersion is found in living matter. Figuratively speaking, if all the chemical substances comprising our organism were in true or crystalloid dispersion, reactions would proceed so rapidly that we would, so to say, live ten years in ten minutes. On the other hand, if coarse dispersion prevailed, it would take ten years to live ten minutes. Every organism is dependent upon the coördination of its chemical reactions *in point of time*, and this leisurely procedure depends largely on *degree of dispersion*, which keeps chemical reaction velocities within certain speed limits through its regulation of free surface and kinetic activity. Life lies between lysis and coagula-

[31] "Some Chemical Aspects of Life," by Sir Frederick Gowland Hopkins. Science, Vol. 78, No. 2020, September 15, 1933, p. 227.

Cf. also "Present Tendencies and Methods of Physiological Teaching and Research," by A. V. Hill. Science, Vol. LXI, No. 1577, March 20, 1925, esp. p. 302.

[32] Cf. "Physiological Time," by Dr. Alexis Carrel. Science, Vol. 74, No. 1929, December 18, 1931.

tion. The colloidal zone is, as it were, a vital metronome tolling off the tempo of life."[33]

To group organisms of like chronological age in scientific work is to ignore this relativity of time for each energy complex and invite unnecessary confusion.

In scientific thinking, time is usually regarded as a variable, but, as the previous discussions and quotations (especially those by Dr. Carrel) have indicated, *time is also a process.* The recognition of this is essential to future biological and social research since the time process, no less than spatial changes and energy transformations, participates in the developments under study. Reluctance to accept modifications in the conception of time is evidence of the deep-seated belief in spatial entities as the only "real things" and of the resistance to the idea of relativity in biological thinking. To accept time as the measurement of a process of varying significance depending upon the stage of development of the organism under study is to relinquish faith in many of the cherished generalizations of biology, especially of physiology, that imply, when they do not explicitly state, that physiological processes are absolute, i.e., independent of the time system of the organisms, their duration or age and previous history.

These criticisms of analytic procedures and of the use of time concepts are associated with the organismic conception, which has been developed as a protest against this parceling out of an organism according to the variety of data which it may yield; it marks a movement toward the explicit recognition of the idea that an existent, such as an organism, will yield as many different kinds of data as the investigators have the techniques for discovering and recording, and that among these data there will be certain relationships arising from the very nature of an organism as an energy complex in a four-dimensional universe, with multiple "fields" in various stages of development. This does not mean that these different data must or should be correlated,

[33] "Enzymes, Vitamins and the Zone of Maximum Colloidality," by Jerome Alexander. Science, Vol. 80, No. 2064, July 20, 1934, p. 80.

since within an organism, especially when growing and developing, the process of change and adjustment and the interactions of "fields" occur with varying degrees of lags and efficiency. The study of individual organisms as they grow and develop is still so new that little is actually known of development.

The analytic tradition of science that insists upon breaking down an organic whole into discrete parts and attempting to correlate two variables is the chief obstacle to the study of organisms. Many investigators assert that the study of an organism instead of the restricted problem of two variables is not scientific, probably because they prefer the abstractions of anatomy and physiology and creating methods for obtaining data and establishing their meaning, which are of course primary to the study of organisms. Here again we see how the study of organic growth and development is impeded by the conceptual framework of contemporary science.

"It is astonishing that, in the face of the advance of the evolutionary method in natural science, any logician can persist in the assertion of a rigid difference between the problem of origin and of nature; between genesis and analysis; between history and validity. Such assertion simply reiterates as final a distinction which grew up and had meaning in pre-evolutionary science. It asserts, against the most marked advance which scientific method has yet made, a survival of a crude period of logical scientific procedure."

"The significance of the evolutionary method in biology and social history is that every distinct organ, structure, or formation, every grouping of cells or elements, is to be treated as an instrument of adjustment or adaptation to a particular environing situation. Its meaning, its character, its force, is known when, and only when, it is considered as an arrangement for meeting the conditions involved in some specific situation. This analysis is carried out by tracing successive stages of development—by endeavoring to locate the particular situation in which each structure has its origin, and by tracing the successive modifications through which, in response to changing media, it has reached its present conformation. (See Philosophical Review, XI, 117–20.)"[34]

[34] "Essays in Experimental Logic," by John Dewey. University of Chicago Press, 1916, pp. 93 and 94.

The organismic conception, while awaiting further clarification, definitely implies a recognition that the process of growth and development, not merely the measurement of structural and functional aspects, is the focus of biological thinking and research.

If the foregoing discussion appears complicated and often clumsily expressed, it should be recalled that the ideas put forth are not yet clarified nor integrated into a coherent body of thought. "Each one can find lucidity only in those ideas that are in the same state of confusion as his own." (Proust) It must be reiterated here that the various formulations and ideas presented in this paper are not offered as verbal solutions of difficult problems, but as suggestions for reformulating problems in more fruitful ways for investigation. For those who are inclined to believe that scientific progress is purely a question of precision in methods with unlimited patience, a remark by Dr. Carrel may serve to conclude this discussion: ". . . in the development of every science, the conception is more important than the method. Techniques are only the servants of ideas. . . . A method is an instrument which finds only that which is being sought."[35]

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[35] "The New Cytology," by Dr. Alexis Carrel; supra.

The Relation of the Attributes of Sensation to the Dimensions of the Stimulus

BY

EDWIN G. BORING

IT IS the traditional view of psychology that the attributes of sensation show a one-to-one correspondence to the dimensions of the stimulus. Some such view is also implicit in the naïve epistemology of the physicist. He often thinks of pitch as if it were the perception of the frequency of a tone, but that view soon runs into difficulties. Within psychology it was Wundt who originally equipped sensation with two attributes, quality and intensity, thus making sensations mirror the more obvious aspects of stimuli which differ as to kind and in degree. Later it became clear to Külpe that extension must be an attribute of visual and tactual sensations because the retina and the skin are areal organs and because extent enters into perception quite as 'immediately' as does intensity. A loud sound is not a congeries of faint sounds, nor is a perceived line a row of sensations. In a similar reaction against Wundtian atomism Külpe also added duration (protension) to the list, for a tone that lasts 0.8 sec. yields a different perception from one that lasts 0.6 sec. and yet there is no sense to saying that the first represents a greater number of seried sensations than the second. Thus the four classical attributes of sensation are quality, intensity, extensity and protensity. The conventional view has been that

they are respectively correlated with four aspects of the stimulus, and this paper is written with the intention of refuting this view.

It may seem strange that orthodox dualism has thus patterned 'mind' upon 'matter,' but we must remember that dualism has, for the most part, also been parallelism, and that the correspondences of parallelism are suggested by functional relations. In the absence of direct knowledge of the system of relationships involved, one looks for some simple and symmetrical preëstablished harmony in the parallelism. Locke placed the primary and secondary qualities in the object, but it is quite usual to think of them as in the mind. The belief that a change of hue is correlated with a change in the wave-length of light is readily transformed into the belief that, in perceiving hue, we are also perceiving the wave-length which the hue symbolizes. Similarly we may take the perception of visual brightness as the perception of its stimulus, the energy of the light. The confusion is easy in any simple, isomorphic parallelism, but there is at the present time less excuse for its introduction into experimental psychology than there was when the knowledge of sensory phenomena was more speculative and less advanced.

There have long been available the data that show the failure of this simple correspondence in the case of visual sensation, but the full significance of the facts has never been grasped. It has been conventional to say that hue depends upon the wave-length of light, brightness upon the energy of light, and saturation upon the mixture of wave-lengths. Thus the dimensions of the stimulus have been regarded as wave-length, energy and mixture, and the three corresponding attributes of the sensation, represented in the color solid, are hue, brightness and saturation. However, this correlation of three-to-three can no longer be considered a correlation of one-to-one. In fact, it is now plain that the three attributes of color would have been required by a complete knowledge of the sensory effects of bidimensional monochromatic light. Without mixture we could not get the purples or the grays, but we should still need three attributes. Hue varies with wave-length when energy is constant, but it also varies with energy when wave-length is constant, as the Bezold-Brücke phenomenon

shows. Brightness varies with energy when wave-length is constant, but it also varies with wave-length when energy is constant, as the 'visibility' curve shows. Saturation varies with energy when wave-length is constant, being maximal at medium energy and minimal for little and for great energy. Saturation varies with wave-length when energy is constant, apparently being least in the region of the yellows and the violets, although these functions are not exactly known. If hue, brightness and saturation are joint functions of wave-length and energy in accordance with different laws, so that they become independently variable, then they can be established as separate attributes, even though this fact means that we are getting three attributes out of a bidimensional stimulus. It is with this general problem that the present paper is concerned.

The problem has recently been brought into the open by the work of Stevens[1] on the attributes of tone. The classical view has been that pitch depends upon the frequency, and loudness upon the energy, of a simple harmonic sound-wave. However, it has been known for some years that loudness varies with frequency, when energy is constant, in the manner indicated by the parametric function in Fig. 1. Both Stevens[2] and Zurmühl[3] have shown recently that pitch varies with energy when frequency is constant. One such relation is shown in Fig. 1, although the form of the function depends upon the particular pitch which is selected as the parameter. These two facts invalidate the principle of one-to-one correlation between the sensory attributes and the stimulus dimensions, but they leave the relationship as two-to-two and thus preserve the temptation to expect symmetry.[4]

[1] S. S. Stevens, The attributes of tones, *Proc. Nat. Accd. Sci.*, 20, 457–459 (1934); cf. Volume and intensity of tones, *Amer. J. Psychol.*, 46, 397–408 (1934); Tonal density, *J. Exper. Psychol.*, 17, 585–592 (1934).

[2] Stevens, The relation of pitch to intensity, *J. Acoust. Soc. Amer.*, 6, 150–154 (1935).

[3] G. Zurmühl, Abhängigkeit der Tonhöhenempfindung von der Lautstärke und ihre Beziehung zur Helmholtzschen Resonanztheorie des Hörens, *Zsch. f. Sinnesphysiol.*, 61, 40–86 (1930).

[4] The precision of the functions shown in Fig. 1 is known. The curves are based on averages. An error appears because the functions for volume and density tend to be ogival about the reference tone, wherever the reference tone may be. However, these

The situation is complicated, however, by Stevens' discovery that he can validate volume and density as attributes of tone quite as readily as he can validate pitch and loudness. Fig. 1 shows a typical set of his results. There the origin represents the frequency and energy of a reference tone of a given pitch, loudness, volume and density. The lines are isophonic contours

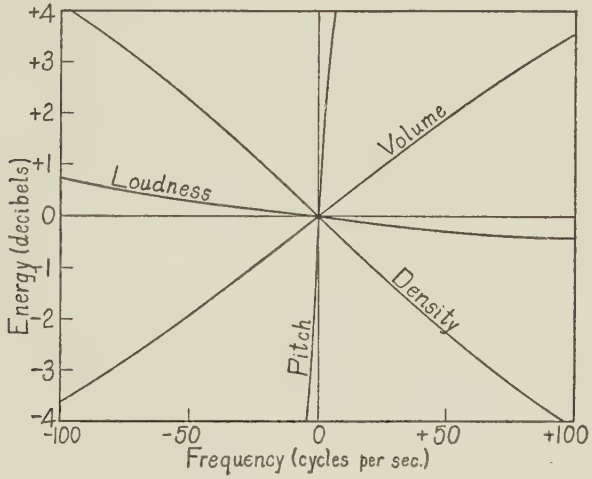

Fig. 1. Isophonic Contours Representing Equal Pitch, Loudness, Volume and Density of Tones Equated to a Reference Tone of 500 Cycles and 60 db. and Plotted against Frequency and Energy of the Stimulus

The reference tone is placed at the origin, and the stimulus values are measured as deviations from it. With a larger scale and a wider range of values the pitch contour no longer appears as nearly vertical, nor does the loudness contour seem to be nearly horizontal. From S. S. Stevens, *Proc. Nat. Acad. Sci.*, 20, 458 (1934). The graphic relationships are considerably altered for other reference tones of very different frequency and energy.

or parameters, showing the values of the two stimulus dimensions that yield constancy in respect of each of these four attributes. The contours are got by direct comparison. The subject is given alternately tones of different frequency, and he varies the energy of one until it equals the other with respect to the attribute in

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functions differ from the form that they would have if entirely independent of the reference tone by an amount that is not large with respect to the probable error of observation. There is probably a small artefact involved.

question. The figure shows that equality in respect of one attribute means inequality in respect of the other three. There can be no doubt that we have here four valid aspects of tonal sensation that depend, according to different laws, upon the two dimensions of the tonal stimulus.

It was this situation that set the problem for the present paper. At first it seemed that Stevens' finding was impossible, that a two-dimensional stimulus could not yield a four-dimensional sensation. That contention is, however, not correct. The sensation is not four-dimensional, because any two of its attributes determine the other two when the laws of relationship are known.

Therefore it may true that, if a two-dimensional stimulus can yield a sensation with four attributes, there is no limit to the possible number of attributes. This view, which appears at first to be a *reductio ad absurdum*, turns out to be essentially correct with two limitations that must be stated presently. Since it is a surprising conclusion in view of the history of psychological thought on this matter, we shall do well to turn from the particular to a more general consideration.

The Bidimensional Stimulus.—Let us consider the general case of the two-dimensional stimulus, where the measures of the stimulus in respect of each of its dimensions are r and s. For instance, r might represent the energy of tone or light and s might be the frequency. These values are, of course, defined by very complex operations, but there can be little dispute about their meaning as they enter into the psychophysical experiment.

Now let $a, b, c, \ldots\ldots$ represent measures of differentiated reactions of the nervous system or of the sense-organ to the stimulus. We may, for the time being, limit our consideration to reactions in the sense-organ, knowing that whatever can be said of the physical system that includes the stimulus and the sense-organ may turn out to be applicable to other consequent systems, like the system in which brain process depends upon excitation in the sense-organ, or the system in which overt muscular response depends upon brain process. We shall see later that the kind of differentiation that is required here to support the existence of separate attributes is a difference between a and b of the sort that

can lead to different neural consequents. Meanwhile we note that spatial differentiation is the most obvious kind of difference for this purpose. If a and b are events at different places in the sense-organ (e.g., the activation of different fibers of the basilar membrane of the inner ear), then the differentiation can be preserved in the consequent events because different nerve-fibers are excited. The main point is that a differentiation once established must be preserved in consequent neural events if it is finally to become the ground for 'conscious' discrimination.

Let us now take the case where each of these reactions of the sense-organ to the stimulus is a joint function of both of the dimensions of the stimulus, in the way that tonal loudness depends upon both the frequency and energy of the tone.
We may write:

$$a = F(r, s) \qquad b = \Phi(r, s) \qquad c = \Psi(r, s)$$

It is plain that complete investigation of these functions would yield a series of relations between each pair of variables, if the third variable is held constant as a parameter:
With s constant,

$$a = f'(r), \qquad b = \phi'(r), \qquad c = \psi'(r)$$

With r constant,

$$a = f''(s), \qquad b = \phi''(s), \qquad c = \psi''(s)$$

Then, with a, b, c.....constant,

$$r = f(s), \qquad r = \phi(s), \qquad r = \psi(s),$$

respectively.

For each value of a there is a different function f. We may write:
for $a = a_n$,

$$r = f_n(s);$$

and similarly for b, c,..... In other words, there is a different function between r and s for every value of a, and a determines the function. However, a does not fix the value of r or s separately, because a given a implies any value, within the limits of

the function, of r if s is undetermined, or of s if r is undetermined. A knowledge of events in the sense-organ yields information about the nature of the stimulus only when the particular functional relations have been empirically established.

There are two further conditions that must be fulfilled if a, b, c, ... are to result in the existence of three sensory attributes. The establishment of an attribute requires overt discrimination by the organism. If the basis for attributive differentiation is to be found in the sense-organ, then (1) the peripheral differentiation must be preserved centrally, and (2) the conditions of discrimination must obtain.

(1) It is impossible to say just what kind of peripheral differentiation can become later the basis for attributive discrimination. Differentiation may be *spatial* (e.g., different regions of excitation on the basilar membrane in the inner ear) or *temporal* (e.g., successive dichotic stimulation of the two ears). When spatial it may be *morphological* (e.g., rods and cones in the retina). Or it may be in respect of the aspects of a spatial-temporal *pattern* (as in most modern theories of hearing, or in Nafe's pattern theory of cutaneous sensibility). Certainly not all peripheral spatial differentiation leads to discrimination. Ordinarily there is no discrimination of the excitations of corresponding points on the two retinas, although there may be such discrimination when retinal disparity determines the perception of depth. On the other hand, there is plausibility in the assumption that different aspects of peripheral excitatory patterns are discriminated in auditory and cutaneous sensibility.

(2) It is necessary further to assert that the events a, b, c, ... vary independently if they are to be discriminated; they must be different functions of r and s, that is to say, f, ϕ, and ψ must not be alike. If the functions are the same, then, regardless of the amount of neurological differentiation among a, b, and c, there will be no independent variation and the differences can not be discriminated. Associated with any a there will always be a particular b, and there arises no possibility of establishing a difference in overt response to a and b. The result is the same whether we appeal here to the logic of independent variability or to the laws of the conditioned reflex.

The conclusion is that the number of attributes of sensation is independent of the number of dimensions of the effective stimulus. A bidimensional stimulus may yield a sensation with one or with n attributes. The sensation will also be bidimensional in the sense that any two of its attributes will then determine all the others, but attributive differentiation may easily transcend in number the dimensions of the system.

Analogue.—An electrical analogue is shown in Fig. 2. An oscillator, S, delivers current, of independently variable voltage, v, and frequency, f, to three amplifiers, a, b, and c. These amplifiers have different characteristics, such that their output voltages, v_a, v_b, and v_c, vary differently as functions of v and f. A is a

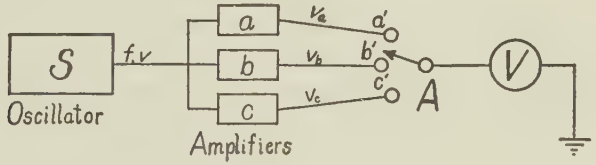

Fig. 2. Electrical Analogue

The analogue of the stimulus is the oscillator, S, which delivers current of variable frequency, f, and independently variable voltage, v. The analogues of the differentiated sense-organ processes are the three amplifiers, a, b, and c, which have different characteristics, so that their output voltages, v_a, v_b and v_c have different functional dependencies on f and v. A is a selector-switch which represents the *Aufgabe* or discriminative attention, and V is a voltmeter representing the ultimate overt discriminatory reaction.

selector-switch and V is a voltmeter that shows one output voltage, according to the setting of A. The analogies are simple. S represents the stimulus; a, b and c stand for sense-organ processes; A is the *Aufgabe* or discriminative set of attention whereby discriminatory response is linked to one process of the sense-organ rather than another; and V is the overt response.

The conscious subject is in the place of the man who controls A and sees V, while f and v vary adventitiously without his knowledge. This man discovers that no reading of V when A is set to a' will predict the reading of V if A is shifted at once to b'; and also that there is no predictable relation between the readings from a' and c' and from b' and c'. Hence he concludes that a', b' and c' tap three different attributes of the system. If there were a fixed relation between v_a and v_b so that the reading from a'

always predetermined the reading from b', then he could not come to this conclusion. He would suppose that he had to do with only one attribute, because v_a and v_b are not independently variable.

Of course, the conscious subject may fail to make all these discriminations even when they are possible. He will fare better if he can be given experimental control of S so that he can work methodically. In fact it is only by way of experimental control of the stimulus that volume and density were added to pitch and loudness as attributes of tone.

Tonal Attributes.—We can see the general relationship clearly in terms of Stevens' speculations about the physiology of tonal attributes. It seems almost certain now that pitch depends upon the position of maximal stimulation of the organ of Corti in the inner ear. The response in this case can not be to the excitation as a whole, but to the position of its peak. It seems probable further that the total amount of excitation in all the excited neurones is the correlate of loudness. If these two facts of correlation are accepted, then it must follow that the organism is equipped with means for responding differentially to these aspects of the total excitation. Stevens suggests that tonal volume depends on the spread of excitation about the peak and that tonal density depends upon the ratio of excited to unexcited fibers within the excited area. These correlations seem almost too simple and homologous to be correct, although Stevens has on his side the fact that the naïve observer comes to the experiment already prepared to use the words *volume* and *density* correctly and an attempt to interchange them would seem to him absurd. At any rate we see that the requirements of theory are that there should be some such kind of differentiation if there is to be discrimination; and we see furthermore that as many as four types of differentiation are already established.

Isomorphism.—The logic of this paper casts suspicion upon isomorphic concepts in general. Common belief has held that sensation and stimulus are isomorphic in the sense that there is a one-to-one correlation between sensory attributes and stimulus dimensions. We have disposed of this notion. With it disappears the notion of one-to-one correspondence between processes

in the sense-organ and dimensions of the stimulus. We may have appeared, however, to be preserving an isomorphic relation between events in the sense-organ and the attributes of sensation, but that appearance is an artefact of simplified exposition. The argument is that differentiation in the sense-organ depends upon the nature of the sense-organ in relation to the stimulus and not solely upon the differentiation in the stimulus. In a similar manner differentiation might occur at any neural level consequent upon the peripheral excitation but antecedent to discrimination. It is impossible to say where and how discrimination occurs, but discrimination is to be regarded neurally as having occurred when the final paths that lead to the specific discriminatory reaction have been selectively activated.

In general, isomorphic correlation between any two points in the physical system is likely to be a miraculous coincidence rather than the rule. These relationships are too complex for us to expect simple symmetry.

The Maximal Number of Attributes.—There remains the question of the maximal number of attributes. Theoretically there is no upper limit to differentiation. A stimulus of one, two, three or m dimensions can have one, two, three or n attributes in the resultant sensation. However, there are practical limits. Differentiation of the sort that can lead to differential neural reaction is not unlimited in the finite organism. One expects the basis for a few attributes but not for many. However, let us suppose that there were many differentiated functions. Let us suppose that the neurology of hearing were such as to form the basis for twenty different isophonic contours in the audiogram of Fig. 1. Should we then expect to be able to plot all these contours? Hardly, for discrimination would break down when differentiation between functions becomes so slight. We can not expect to establish a larger number of attributes that can have clear and definite differentiation from one another with regard to their functional dependences upon the stimulus. It is always easier to discriminate the few than the many.

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The Forgotten Problem: Aims in Science

BY

JOHN SOMERVILLE

AT THE present time there is a tremendous discussion of the scientific method. But there is almost no discussion of the scientific aim. This is a paradoxical state of affairs. Is it possible to discuss method intelligently unless the question of aim is first given thorough and serious consideration? Is not a method, by virtue of the very fact that it is a method, something which is subordinate to an aim and determined by that aim? Is not a method something which is to be understood, evaluated, attacked and defended only in relation to the particular aim which it serves? Surely to deal with method in any other way is to mishandle the situation. Yet at the present day we witness this tremendous volume of discussion of method, with almost no discussion of aim.

It is not my intention, in raising this problem, to voice the familiar, indeed the hackneyed complaint that in science we are developing a great deal of control over natural forces without heeding to what end we are applying these powers, or to reiterate that we are going somewhere very fast, but that we do not know or care where. Such is not my problem. My problem is not the end to which we ought to apply the *fruits* of knowledge. It is rather the necessity of clarifying to oneself the kind of *knowledge* he is aiming at before discussing questions concerned with the method of attaining it. It is first of all a problem in logic. This

logical problem, however, is related to the other, which is a moral problem. Its solution is really a condition of the solution of that moral problem.

Now how are we to account for the fact that while the methods of science are being given the most intense concentration, the most elaborate analysis, the aims of science are hurriedly passed over with merely a casual glance? There are of course a good many answers that might be made to this question. Let us here examine the one that is found most frequently returned when the question is put, namely, that the aim of science is more or less obvious, being just the discovery of truth or knowledge, and that in consequence of this fact, we may, as it were, take the aim for granted and immediately pass on to a discussion of methods.

It does not appear that this position is capable of being justified. It may be maintained as true, but it cannot be justified as adequate. For instance, it is not only the scientist, strictly so called, who is seeking "truth," but every intellectual investigator, of whatever kind. Thus, according to this answer, we are to conceive of the geographer, the grammarian, the biologist, the physicist, the metaphysician, the statistician and the theologian as being unanimous in aim because they are all seeking for the truth. We have to consider that all these people have the same aim, but in some more or less arbitrary, perhaps temperamental manner, they have worked out different methods of reaching that identical goal. In this view the differences of method are not traceable to difference of aim. They remain in the nature of brute facts which as phenomena of logic are given no further explanation.

People who take the view that scientists do not have different aims might also be expected to take the view that scientists do not have different methods. Indeed they often do take this view which crystallizes the situation by carrying the position to its logical termination.

Now there is no doubt whatever that there is something in common to the aims of all scientists, or all sciences. This is quite clear, else the aims and the sciences would not all belong

to their respective classes of "aim" and "science." And the very same thing holds true of methods. But to say that two aims or two methods have something in common is not to say that there is no difference between them. While they have certain elements in common, they have certain elements of difference. You and I, for instance, have something in common. We also have important differences. It is not too much to ask that anyone who seeks to understand you and me should take account of these differences.

In science we have precisely the same situation. It does no good to insist that because all methods have something in common, they therefore have no differences, because in that way the differences which they do possess do not receive the proper consideration. For the same reason, it does no good to insist that because all aims have something in common they therefore have no differences. It is not unreasonable to hope that the situation can be clarified by tracing differences of method to differences of aim. And it is the differences of method, we may remark in passing that excite most of the controversy, rather than the common factors.

In urging this point I am not unmindful of the fact that the scientific study of any class of objects will be a study of those things which the members of that class have in common. For instance, no one will deny that to study animals as a class will involve studying what all animals have in common. But how inadequate the study of animals would be if it stopped at that point. There is nothing to prevent it, in fact, there is everything to encourage it, to go on and form sub-classes of animals and study each of these sub-classes in terms of what *its* members have in common. Now in order to break up the large class animal into various sub-classes, a proceding absolutely indispensable if the study is to be taken out of its elementary stage, the differences of animals must be observed and analyzed. In fact, it is in this way that the class animal is itself formed, as a sub-class under the class living things, which in turn is a species of the genus material things and so on. Thus while the scientific aims of all scientists might be put in one class, by virtue of their

common pre-occupation with truth, this of itself sheds no light whatever on the problem of setting up sub-classes.

In general, we may say that there are several levels of problem. There is the question what, if anything there is common among:

1. The aims of all men whether they be scientists or not,
2. The aims of all scientists,
3. The aims of all natural scientists,
4. The aims of all social scientists,
5. The aims of all chemists,
6. The aims of all sociologists,

and so on. Of course, some of the most important differences of aim are those that cut across previously constructed lines of division, and simply do not follow the well known borderlines of the sciences. Again, such questions will be fruitfully discussed not only in terms of what these aims are, but of what they should be. In general, if every battle over what methods should be were preceded by a conference, or a battle if you wish, as to what aims should be, the situation would be very greatly clarified.

Thus no matter what ultimate view of truth we may have we will all admit that there are in the first place different kinds of truth about the same thing, as for instance, metaphysical, ethical, chemical, sociological, and that there are in the second place, truths about different things.

Even such obvious and simple differences as these bespeak differences of aim as among the sciences, which ought to be taken account of in discussing scientific method. Then again, still in the realm of the obvious and simple, some scientists or sciences may have as their aim the discovery of causes, and among these some may seek a first cause while others may reject the aim of a first cause. Among others there will be disagreement over the question whether final causes are a proper aim. Others again will reject the concept of cause in any form, and seek for laws. But here also we must not be led into thinking that because the same word is used, the same object is in view. Insofar as the concept of law is different, the actual aim is different.

But these differences of aim so far referred to are, as I have said, obvious and simple. The more complex differences, those

that make the problem really interesting, are found beneath the surface and are not outwardly distinguished by differences of terminology. These are differences of the very nature and essential character of the knowledge which is sought, and they require more than any other to be brought out into the open.

For purposes of illustration, let us analyze the contrast between two such aims found in the history of philosophy—that of Plato (assuming that we may take the utterances of Socrates in the "Dialogues" as the opinions of Plato) and that of Francis Bacon. This contrast is to a certain extent demarcated by terminology. That is, Plato sought for the knowledge of what he called Ideas, while Bacon's aim was what he called Forms. Bacon's aim was the kind of knowledge that yielded power over the phenomena with which it was concerned. The power which he had in mind was in general the power of prediction, utilization and control, the power, as he put it, to "work works." He held fast to such an aim, not because he revered power or utility above knowledge —in a remarkable passage[1] he specifically says the opposite— but just because to his mind, this ability to lead on to power, fruit and works was a mark and characteristic of genuine knowledge. He meant it to be used as a criterion to test the relative claims of different doctrines or alleged sciences. A handy way to do this was to inquire what power, if any, the pretended knowledge was able to yield over the phenomena with which it was concerned. To the extent that it yielded power, it was genuine scientific knowledge.

Such was the aim that made Bacon's scientific method what it was. In Plato on the other hand, matters are quite different. He does not seek the kind of knowledge that will lead to power. The power to predict and control phenomena is for him no criterion of the validity of knowledge concerning these phenomena. The knowledge he seeks must meet the conditions of a perfect definition by way of organizing and clarifying that which we already know without sending us abroad, necessarily, to accumulate more data. Plato's aim was not to delve deeper into the empirical characteristics of things so as to predict and

[1] Novum Organum, Book 2, Aphorism 124.

control their behavior. While the knowledge which he sought had its uses, it was above all for abstract contemplation rather than for material use.

As between Bacon and Plato, we have no absolute contrast of utility and contemplation. Bacon was for contemplation as well as utility. In fact he goes so far as to "assent" that the "contemplation of truth is a thing worthier and loftier than all utility and magnitude of works."[2] However he insists that the truth to be contemplated be genuine truth, and the only way to make sure of that, in his view, is to test it by the criterion of power. "Truth therefore and utility," he goes on to say "are here the very same thing: and works themselves are of greater value as pledges of truth than as contributing to the comforts of life."

Plato on his part is not solely for contemplation. Especially notable are the political and moral uses of knowledge, as we see in such dialogues as "The Republic" and "The Apology." But Plato does not make the power of prediction and control over the sensible phenomena a necessary characteristic of knowledge.

In the end, the contrast is a matter of degree and emphasis. This fact, of course, does not prevent it from being of tremendous importance. In fact, in broad terms, this contrast characterizes the essential difference between the modern aim of scientific knowledge and the aim of the Greeks. The difference between the Greeks' pursuit of knowledge and our own represents a shift not only in the methods used but in the aims held. As a matter of fact the difference in method is a natural and necessary concomitant to the shift of aim.

The problem of aim becomes especially important in the construction of a new science. One of the most important intellectual endeavors of our times, a matter which has been the concern of philosophers as much as scientists, is the attempt to create the science of sociology. It is not too much to say that when and if sociology becomes a science which has a relation to its subject matter such as the sciences of physics and chemistry have to theirs, a new epoch in human history will have begun.

[2] Ibid.

Now sociologists and social scientists generally are alive to the problem of method. They appreciate to what an extent the question whether their fields can ever become truly scientific hinges on methodological matters. But they do not appreciate the extent to which problems of method hinge on aims. It does not appear that they are alive to the problem of aim. It would seem for the most part that the sociologists are prone to go blithely forward under the general impression that their aim is the truth about society and that their only real difficulty is that of method. The consequence of this situation is a half unconscious feeling that any type or kind of knowledge so long as it is about society and so long as it is "true," will do. As a result the social sciences become swamped with a perfect deluge of every conceivable type and kind of information, statistics, trends, findings, facts and figures, much of which is utterly useless for scientific purposes. An indiscriminate collection of knowledge, however true and however faithful it may be in restricting itself to society, simply does not constitute a science of society.

Because there is no real unity of aim, there is no unity of method, there is no proper organization of problems, and there is no scientific integration of the various projects and pieces of work that are done here, there and everywhere. The whole field is cultivated in a fragmentary, one might almost say an eccentric manner. The very terms and units of whole troops of investigators are not only different, but utterly incommensurable. If one may be permitted to say so, there is a lack of feeling on the part of social investigators as to what constitutes a scientific problem.

It goes almost without saying that all these questions—unity of method, the organization and integration of problems, standards of terminology, the question what is a problem, would be very greatly illuminated if social scientists would come to grips with the question of aim, the type of knowledge they want (for instance, the Baconian, the Platonic or something else) and the relationship they expect it to have to the existing subject matter, instead of passing over these questions in the belief that all aims

are the same, only to find every subsequent step beset with discord and confusion.

In like fashion the more fundamental question, what is science as distinguished from other kinds of knowledge, and what is the scientific method as distinguished from other methods would receive considerable clarification if first dealt with on the level of aims. The very same considerations apply to questions of philosophic method. Does the history of philosophy show that all philosophers have the same aim and the same method, or does it show that all philosophers have the same aim and different methods, or does it show perchance that they have similarities of aim and *hence* similarities of method, and at the same time differences of aim and *hence* differences of method.

It is of course perfectly possible to be influenced by an aim and to be serving it without being aware of the fact. This is what happens when thinkers commit themselves to a certain method without inquiring into the problem of aim. They are then sometimes very greatly surprised at the results and consequences of the method. They may attempt to console themselves with the reflection that they are pure scientists, but no one would consider an individual remarkable for his wisdom if this individual let us say, assisted biologically at the birth of a child, but then protested that he had no such ultimate result in mind, but was in fact a pure lover, being occupied with the method of love making for its own sake. Such a man is not praised for a saint, but condemned for a fool. Commitment to a method is commitment to an aim.

There is a path through which the whole question leads directly into morals. While we cannot follow it there we may simply note that scientific aims, in the theoretical sense, are often chosen on the basis of their fitness to contribute to certain further aims of a moral, political or metaphysical nature. That situation constitutes another level of the same problem. It may be remarked in passing that insofar as the moral problem itself is concerned, we find the same puzzling phenomenon as in the logical problem, namely, the unwillingness of men to reconcile

themselves to the fact that moral aims have differences. Here the word happiness often plays the same ambiguous and confusing rôle as the word truth. A more thorough consideration of the rôle of aim should in these fields have the same clarifying effect as it would in the philosophy of science.

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DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

FLEXIBILITY OF SCIENTIFIC TRUTH

Dear Sir:

I should like to point out that the "flexibility of scientific truth," discussed in the July and October issues of this journal by Messrs. Zwicky and Marganau, is a corollary of Peirce's Synechism or Law of Continuity which was briefly explained in my article in the October issue (pp. 448ff.). That such flexibility is not at all equivalent to "universal doubt" will probably be clear to anyone who reads Peirce's argument in his *Collected Papers*, vol. 1, pars. 137, 141–175 (see also his article on "Synechism" in Baldwin's *Dictionary of Philosophy and Psychology*, which will be republished in the *Papers*, vol. 6, 169–173). Rather than scepticism it is perhaps even better taken as a positive belief in the variety and creativity of nature, which is too full of individuality and novelty to fit precisely any law of repetition or duplication. Mr. Marganau's example of absolute truth states no such law of nature apart from man, and is not an instance of what is usually classified as a scientific *truth*. (It is rather the merest *fact*.) Nor is it absolutely certain, since not all the copies of the July issue can be known beyond shadow of doubt to have been printed from the same type, or, upon any evidence, to be exactly alike in the respect in question.

Peirce did not deny that there are indubitable beliefs. But he showed that they are all vague, commonsense beliefs, not scientific doctrines (5.446 or *The Monist*, vol. 15, pp. 481–99—Character IV). The moment we try to give a precise analysis of anything we cannot doubt we find we *can* doubt whether we have given the right analysis. The only indubitable precision is in pure mathematics, and even there there is always

a slight chance we have made a slip. But this chance is often negligibly small, which is all we need to escape scepticism of the self-refuting kind.

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SPATIAL LOCATION AND THE PSYCHO-PHYSICAL PROBLEM

My dear Editor,

I have just read Professor Feigl's *Logical Analysis of the Psycho-physical Problem* (this Journal, October 1934) with a mixed feeling of pleasure and disappointment. I was pleased to observe a logical positivist tackle a specific epistemological problem, such as the localization of a sensed patch of color (p. 440), with the promise of either analyzing the problem out of existence if nonsensical or of solving it if significant. My disappointment arose from the fact that, so far as I can see, he did not keep his promise.

In the language of pure descriptive psychology (or "data"), obviously a patch of red is "precisely where I see it" (p. 440) or exactly where it appears to be in "visual psychological space." But, to the epistemologist, the important problem is how to locate the visual sense-datum if not literally in, at least with reference to, physical space. Professor Feigl says that physical space "logically analyzed is a construction and correlation of a manifold of data given in the various psychological spaces" (p. 441), and that is the only hint he gives us as to the place of colors in physical space. Does this mean that, since we speak significantly of colors only in the "language of data," and of physical position only in the "language of constructs," we cannot even significantly ask about the position of a color (datum) in physical space (construct)? If so, I suppose the same is true of positions of "psychological spaces" in physical space. Are we to say that, for example, my psychological space is neither in nor not in physical space, since we cannot significantly even raise the question? Are only physical constructs (compounds of data, not simples) to be located in physical space? I believe that Professor Feigl answers these questions in the affirmative, as for example Phenomenologist Price does in his book entitled *Perception*. But an affirmative answer—besides conflicting with the statement that one language is translatable into the other—raises a problem, which I shall state in conclusion, in the hope that someone will lead me out of the dark into the light of understanding.

Professor Feigl asserts that in the language of data or from a radically empirical point of view, the question about the evolution of consciousness and animal life cannot be raised. Only in the language of constructs is the question significant and answerable, the physical space and presumably also time in which such evolution occurs being themselves constructs. Now, my problem is this: what does the constructing of a construct? What constructs constructions? It seems to me that this constructing agency would be a *conceiving* (mental) or logical operation of some sort, such that physical space and time, according to positivism, are *resultants* of the kind of activity we call mental. In short, before there can be physical space and time—the conditions of evolution—there must be logical or mental agency. And, if the positivist is right, the question of the origin of consciousness itself (the *constructing* agency) cannot be significantly raised in any sense whatever, *particularly not in the language of constructs*. How then are we to speak significantly of the evolution of consciousness or of that agency which constructs physical space and time?

Will Professor Feigl help to see the light by answering these questions one by one? I have a genuine suspicion that my critical questions are terribly old-fashioned, perhaps even nonsensical. In which case, I want only to be shown why I should never have asked them.

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Houston, Texas.

Dear Sir:

Professor Aldrich's question concerning my view of the location of a color-datum in *physical space* involves the danger of the very confusion of syntactically incongruent languages which I was so anxious to eliminate. The hopeless condition of traditional epistemological discussion, especially as regards the location of sensa, and more generally, the relation of the knowing subject to the known objective world, etc., can be overcome only by closer attention to the ways in which "languages express meanings," i.e., to the syntaxes of our conceptual systems. If two languages are as heterogeneous as the Language of Data and the Language of Physical Constructs, greatest care must be taken in transferring concepts typical of the one language into the other. The two languages do not have as simple a relation of one-to-one correspondence as, for example, English and German so nearly exhibit. But as there

is the possibility of translation it can very legitimately be asked what the physicalistic equivalent would be for a proposition describing the location of a colored patch in visual space. Translation in this case would yield a proposition dealing with the condition in the nervous system, which as we usually say, "is the neuro-physiological basis" of color and space-perception. That the detailed nature of this condition is scientifically still to be determined detracts nothing from the practical certainty that it *is* a proposition concerning neurophysiological processes in my body which is the translation-equivalent of the introspectively established and phenomenologically formulated proposition about my color-experience. To use for a moment the less stilted idiom of ordinary scientific realism, we mught justify the statement just made as follows: In order to see a color it is not necessary that there be an extradermal stimulus. Eidetic imagery, dreams, hallucinations, and other "abnormal" forms of visual experience prove sufficiently that there can be color-data located in visual space phenomenally very nearly, if not exactly, like in "veridical," "normal" perception. That is one of the evidences, practically cogent to the scientific mind, for the doctrine of the localization of our sensations within our organisms (not necessarily in the brain only). The ensuing difficulties of a reconciliation of this *scientific* view with the equally cogent, apparently contradictory claims of immediate experience constitute a problem of long standing. The first more decisive steps in the right direction toward a solution seem to me to have been made by Avenarius, Poincaré and then by some critical realists (viz., the epistemologically dualistic but psycho-physically monistic group) especially Riehl, the earlier Schlick, Strong, Drake, and from the point of view of psychology, Koehler. Most recently an excellent discussion, also in connection with a critique of Lovejoy's revolt against psycho-physical monism, has been given from this angle by the brilliant French critical realist R. Ruyer in his article, "Les Sensations, sont-elles dans notre tête? (*Journal de Psychologie*, XXXI, 7–8, 1934, p. 555ff.) That minimum of ontological metaphysics, which even the most critical of critical realists (Schlick until 1925 and Ruyer) have not been able to avoid has been successfully eliminated (or transformed into legitimate expression) by the New Positivism. This view, as already indicated, provides for complete intertranslatability of phenomenal and physical language—in our problem the intertranslatability of the geometry of visual space and that of physical space.

The question, Is visual space located in physical space? has at best a vague pictorial appeal, but is, as Mr. Aldrich himself suspects, *factually*

meaningless. A study of the syntactical structure of the question reveals this very clearly. The relations of spatial inclusion are defined *within* the syntactical frame of both geometries. But to ask whether the whole of visual space is spatially included in physical space is nonsensical because it violates the logic of types (the unproblematic part of Russell's theory). Although I am aware of the dangers in the use of analogies, I will illustrate the crucial point by an analogy which in this one respect seems to me quite pertinent. As is fully appreciated today the *geocentric system* (not the original one of Ptolemy but its complete and adequately corrected form) and the *heliocentric system* (again not the historical one of Copernicus but its modern corrected version) are *equivalent*—at least as far as kinematics goes—descriptions "of one and the same fact." They differ in the choice of the coördinate system—but there are definite transformation equations by means of which we can translate the geocentric language into the heliocentric language. But as the choice of the coördinate system in this case involves rather striking differences in the "organization" of the facts the perfect logical equivalence of the two descriptions was not immediately recognized. (People who held that the heliocentric view was true were persecuted.) From our modern viewpoint—after Einstein has taught us that the two systems are also dynamically, physically equivalent—we see that the only relevant difference of the two views lies in the greater conceptual convenience, the greater descriptive simplicity of the heliocentric system. My point now is, of course, this: A philosopher might argue, How can the two systems describe the same fact, if the orbits of the planets are so different in the two systems. He might ask to be shown *where* exactly Ptolemaic epicycles are to be found in the heliocentric system. Well, it so happens, there are none there. The point of the analogy is clear: The different organization of the "facts" (not a third unknowable entity but the thing "known as" one or the other "perspective") as they are rendered in the syntactically incongruent languages provokes the pseudo-problems of psycho-physical dualism. Of course, the color as seen, and as described in the phenomenal language, can be found neither in the brain-as-seen nor in the physical surface properties of the perceived object. Now, that in the ontogenetic (and possibly phylogenetic) experience of the eye-equipped organisms a coördination develops between the motor-tendencies directed toward parts of the environment and the visual receptor processes is as good a scientific fact as any. It is precisely this coördination which is the physicalistic equivalent to the immediate experience of the adult mind ("color-out-there"). It is very

regrettable that this by no means mysterious situation has been obscured by the use of the unfortunate concept "projection"—which if not very carefully handled (Strong, Drake, Santayana and others are not quite free from misleading formulations of their otherwise very sound view of the matter) stirs up hopeless pseudo-problems.

I hope that these, necessarily sketchy, observations will convey some insight into the reason why psychological and physical spatiality, although correlated (intertranslatable) must not be confused by violations of the rules and definitions that establish their very meanings. In other words, questions regarding their mutual relation can be asked only under full consideration of the key of translation.

This same caution is indispensable also in tackling the problems stated in the concluding part of Professor Aldrich's remarks. Before asking questions it is necessary to make sure of the "reference system," "the universe of discourse" (i.e., the language) relative to which our question makes sense. As we have just seen, although there may be logical equivalents to such questions in several other languages, "*the*" specific question (in its characteristic conceptual expression and emotive connotations) is meaningful (i.e. in accordance with the syntax) only in one of them. To Professor Aldrich's problem (Who or what does the construction of the constructs?) at least two interpretations can be given. (1) It may be meant as a question within the realm of "*logical analysis*" (i.e., the science of logical syntax). There the question is indeed out of place (strictly meaningless), because the only type of problem with which syntactical studies are concerned are the logical relations of propositions, concepts, hierarchies of logical levels, etc. (2) It may mean the *psychological* question as to the agency and procedure of our intellectual adjustment to the surrounding world. Here it makes perfect sense, and the answer can be given on the basis of the results that the psychology of learning and thought has as yet achieved. This answer can then be expressed either in the language of data or (preferably) in the language of constructs (i.e., in terms of biological, bio-physical, and bio-social concepts): It is the human organism acquiring certain verbal habits and manners and tendencies of action which is the "agency that constructs logical constructions."

But here Professor Aldrich will say: "Organism," "habits," "actions" —all these are logical constructions, too—who constructed these? Must I point out that we have here the confusion of distinct realms of discourse? Has the scandal of Psychologism in Logic not taught us, that we must not confuse a purely logical question, such as regarding the

order of logical constructs, with the empirical one, as to the origin of those mental activities which are the vehicles of logical operations?

That the whole physical world in space and time, *in the context of logical analysis* "is" a logical construction of a rather high level of abstraction with its basis in immediate experience is not in the least incompatible with the fact that *in the light of scientific investigation* the constructors of those constructions are small parts and late products of that physical world. Each view is correct in its own right and the question "Which is really true?" is just as profound as the question "Ptolemy or Copernicus?"

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REVIEWS AND NOTES

Relativity, Thermodynamics and Cosmology. By Richard C. Tolman. Oxford at the Clarendon Press, 1934. Pp. XV + 502. $8.50

This book is a very clear, logical and comprehensive treatment of the special and general theories of relativity and their extension to all the important domains of macroscopic physics, including thermodynamics and cosmology. It is worth while emphasizing the inclusion of thermodynamics and cosmology, since in the older texts these topics are either omitted or treated very briefly, the most important developments in both these subjects having taken place in the last eight years, in thermodynamics due largely to the work of Professor Tolman himself. The chapters dealing with these topics are among the most interesting and fascinating in the book, especially to one who is philosophically inclined. The applications of relativity to quantum theory and statistical mechanics are not treated, since the author wished to limit the treatment to macroscopic physics. There is also no discussion of unified field theories, of which the author says, "these attempts (*i.e.*, at constructing unified field theories) appear either to be equivalent to the usual relativistic extension of electromagnetic theory as given in the present text, or to be—although mathematically interesting—of undemonstrated physical importance. Furthermore, it is hard to escape the feeling that a successful unified field theory would involve microscopic considerations which are not the primary concern of this book."

The point of view throughout the book is physical rather than mathematical, or, to quote the author himself, "the endeavour will be made to emphasize the physical nature of assumptions and conclusions and the physical significance of their interconnexion, rather than to lay stress on mathematical generality or even, indeed, on mathematical rigour." One of the features in the mode of presentation that will appeal especially to the logicians and philosophers is the fact that the author states explicitly all the more important postulates and assumptions and discusses their validity on the basis of the experimental evi-

dence, and that he recognizes time and again the uncertainty involved in extending the known laws of physics into new domains, where the experimental data are either lacking or meagre. Furthermore, he recognizes the fact that future changes in the structure of theoretical physics are inevitable, and that this applies to the theory of relativity also. "Nevertheless," to quote the author, "the variety of tests to which the theory of relativity has been subjected, combined with its inner logicality, are sufficient to make us believe that further advances must incorporate enough of the present theory of relativity to make it a safe provisional foundation for macroscopic considerations." His attitude throughout the book is one that can be highly commended from a philosophic point of view.

We shall now present the chief conclusions of a novel character that result from the application of relativity to thermodynamics, especially those of philosophic interest. The chief item of this kind in the case of the special theory of relativity results from the application of the law of equivalence of mass and energy, namely, $E = mc^2$, where E is energy, m is mass and c is the velocity of light. This enables the author to discuss the thermodynamics of the formation of helium out of hydrogen (an example of a nuclear reaction) and to treat the problem of equilibrium between matter and radiation. The conclusions arrived at, however, on the assumption that thermodynamic equilibrium prevails are in neither case in accordance with the known facts, which necessitates a discussion of possible explanations for the discrepancies. In the case of the equilibrium between hydrogen and helium the author finds that, "hydrogen would in any case be almost completely transformed into helium at equilibrium under all but the most extreme conditions of high temperature and low pressure." This is not in accordance with the observed concentrations of hydrogen and helium in the universe. In the case of the equilibrium between matter and radiation the author finds that the concentration of matter should be exceedingly small even at temperature of 10^9 degrees centigrade in spite of the fact that the concentration of matter should increase with temperature. This also is not in accordance with the observed facts. A possible answer, as the author points out, in the case of the problem of the equilibrium of matter and radiation, is that matter and radiation are not interconvertible, as assumed, or at least that there are important limitations on this interconvertibility. Professor Gamow apparently believes that positrons and negatrons (*i.e.* negative electrons) are convertible into radiation but that the more massive particles, such as protons and neu-

trons, are not. A number of other possible explanations are considered by Professor Tolman which are not mentioned here, since to do so would make this discussion too long.

We shall now present the chief conclusions obtained from the application of the general theory of relativity to thermodynamics. The first item is that in a large body in thermodynamic equilibrium the temperatures would not be uniform, but would vary from point to point with the gravitational potential. Another item is the possibility of the occurrence of reversible processes at a finite rate. According to classical thermodynamics all processes taking place at a finite rate are irreversible, but Professor Tolman comes to the conclusion that there is a possibility in relativistic thermodynamics of the occurrence of processes reversibly and at a finite rate. This is of importance in connection with the theory of the expanding universe. The third item of importance concerns the much discussed question as to whether our universe is "running down." According to classical thermodynamics the entropy of the universe tends towards a maximum, at least if the universe is considered to be a closed system, as it usually was in classical thermodynamics. According to this idea the universe would some day reach its state of maximum entropy and since there would then be no longer any available energy the universe would be, from the macroscopic point of view, a dead world. Professor Tolman finds that general relativity permits the possibility of the occurrence of irreversible processes without reaching a final state of maximum entropy. This result is due to the fact that cosmological models can be constructed in which there is no limitation on the proper energy, or, to quote the author himself, "From the relativistic point of view on the other hand, since cosmological models can be constructed which have no limitation on total proper energy and hence also no limitation on total entropy, we must retain the possibility for irreversible processes which may continue without end."

The last chapter deals with the application of the general theory of relativity to the universe as a whole. Historically, the first attempts in this direction were made by Einstein and de Sitter who constructed static models of the universe. These had the property of being spatially finite but unbounded. However, they were found to be unsatisfactory from the theoretical point of view and not in accordance with the observational facts of astronomy. This led to the introduction of non-static models which have been found to be theoretically sound and capable of being adapted to the astronomical facts, the chief fact being

the red shift observed in the case of the distant nebulae. The theory for the non-static models is presented in a very logical manner. The author first of all makes the assumption (common to all cosmological discussions) that all the matter in the universe is smoothed out into a fluid of uniform density, since it would be impossible to take into account the detailed structure of the universe. This, together with other assumptions, enables the author to deduce the form of the line element (*i.e.* $ds^2 = g_{ik}dx^i dx^k$). The results obtained from this line element are not unique, however, due to the occurrence of constants which must be evaluated experimentally, the present astronomical data being insufficient to evaluate these constants to any degree of accuracy. The conclusions concerning the universe are indefinite except for the one result that the universe must be either expanding or contracting. The observational data compel us to take the latter alternative at least for the visible part of the universe. In the present state of the subject it is impossible to decide whether the universe is spatially finite or infinite (closed or open universe). Although the data indicate that the universe, or at least the part we are familiar with, astronomically speaking, has been expanding for 10^9 or 10^{10} years, still one cannot conclude, according to the author, that the universe came into existence 10^9 or 10^{10} years ago. The extent of the universe in time may well be from minus infinity to plus infinity.

A word should be included about the significance of the term "expanding universe." One must guard against the erroneous impression that the expansion in size applies to all objects in the universe, so that our measuring rods would expand in the same ratio in a given time as the distances to be measured. This would leave the numerical value of the length unaltered. Such an expansion would therefore be undetectable unless there were some other physical effects that could be observed. The correct point of view is the following. The general theory of relativity is essentially a gravitational theory, which for small distances gives to a high degree of approximation the same results as Newton's theory. However, when applied to the universe as a whole it gives rise to effects which have no counterpart in Newtonian theory. One of these is the recession of distant objects, which must be looked upon as a relativistic gravitational effect.

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Aristotle's Theory of the Infinite, by Abraham Edel, Ph.D. Privately printed, New York, 1934, pp. 102. For sale at: The Journal of Philosophy, New York City. $1.00

In this monograph Dr. Edel has presented a careful and systematic account of Aristotle's analysis and use of the term "infinite." The strictest meaning, and hence the one fundamental for philosophic usuage is: "of that which admits of being gone through, though the process has no termination, i.e. what permits an endless passage through it." "The emphasis on process, especially as incomplete, points to the infinite as potential rather than actual." (p. 13) The analysis of potentiality reveals three types: the first corresponding to activity (in its restricted sense); the second to movement; and the third, the potentiality for a series involving repeated action. The first two have this in common, that they can be fully actualized, but the third cannot. Hence it is the third type that affords the only kind of existence that the infinite can have. "Infinity is immanent; it is a concept which has meaning only within a finite whole, by that whole being such as never to be exhausted in certain process which is repeated within it." (p. 55) "Especially important is [the] emphasis on a definite subject as a part of the structure. This seeming paradox, that the determinate is prior to the infinite is perhaps the essential point of his account. . . . Infinity is a character of the determinate, a concept applicable only within a closed realm and not even applicable to the realm as a whole." (p. 65) The finite, as distinct from the determinate, means the stopping short where there is potentiality for going on. Loose usage, however, often uses "finite" where "determinate" is meant. "When [Aristotle] calls the world finite he does not mean that it entails a stopping short when it is possible to go on. For on his view it is not possible to go on farther, since the world is the whole and the whole is everything. To speak of the world as finite must mean for him, therefore, to assert its wholeness as an interconnected system, in an inner reference to which alone, concepts such as infinity, space, time, etc. have meaning. It is, in short, the assertion that the world is determinate, that it is a subject of discourse." (p. 72) A chapter on Aristotle's application of his theory to various problems, and one offering a general discussion of his analytic method, conclude this excellent study. Throughout there is presented full documentation and detailed discussion for the interpretation advanced.

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The Two Poles of the Mind, by G. Mannoury. Libraire du Travail, Paris, 167 pp.

In the brochure, Les Deux Pôles de l'Esprit (The Two Poles of the Mind), G. Mannoury carries on a twofold study centering about the subject of lingual psychology. The first phase of his study is a critical, introspective analysis of the meanings of words or terms coupled with an effort to demonstrate their inadequacies as media of thought expression. Here Mannoury seeks to distinguish between the objective and the subjective values of words and to establish the relation subsistent between these values. In the field of objective thoughts or concepts—in Mathematics and in Physics—, he finds notions such as the untuition of time, the present moment, the act of enumeration, the geometric "point," the relation of equality or of "greater than," "nothing" or "zero," to be brimming in ulterior emotional or subjective content. The inadequacy of the word representing an objective thought is found to consist then in its failure to account for the subjective elements or feelings that underly the concept. Similarly, in the field of subjective thought, as in religion where feelings predominate, there is equally bad correlation between the subjective and objective values of words. Among words expressing confessions of faith, for example, there is complete independence of objective and subjective value and, consequently, an inherent inadequacy. Both the objective element in thought as represented by Mathematics and the subjective element as represented by mysticism are devoid of content when they stand alone. The function of language with respect to these two poles of the mind is to serve to establish an equilibrium, not a dichotomy, between the two.

The second phase of Mannoury's study is concerned with the bearing of lingual psychology on communism, the latter term being employed to indicate a social attitude and not necessarily a set of political and economic doctrines. The path by which a connection is established is rather devious. The strife of ideas, beliefs and intentions as revealed by lingual psychology seems to be intimately related with the general life struggle, the struggle of the individual against the community or against humanity, that communism strives to resolve. The current language makes for strife because its component parts are words that are codified, ephemeral and perishable. This language that purports to be for the community is constructed from patches of the language of the individual, the language in which the individual, born a solipsist, became "accustomed to express himself on what is palpable and on what can be counted as known." But Mannoury is confident that man is a social animal, or will, at any rate, yet be one, and he prophesizes that

in the coming era of socialization of spirit, a new language will arise characteristic of that era, a language that will link the particular to the general, the mathematical to the mystical.

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BOOKS RECEIVED FOR REVIEW

God, by John Elof Boodin. The Macmillan Co., 1934. $2.

Three Interpretations of the Universe, by John Elof Boodin. The Macmillan Co., 1934. $3.

Studies in the History of Ideas, by the Department of Philosophy of Columbia University, Columbia University Press, 1935. $4.25.

Alchemy Child of Greek Philosophy, by Arthur John Hopkins. Columbia University Press, 1934. $3.50.

Science and Sanity, by Alfred Korzybski. The Science Press, 1934. $5.50.

Physical Theory, by Victor E. Lenzen. John Wiley and Sons. $3.50.

Atomic Physics, by the Physics Staff of the University of Pittsburgh. John Wiley and Sons.

Economic Control of Quality, by W. A. Shewhart. D. Van Nostrand Co. $7.50.

A System of Logistic, by Willard Van Orman Quine. Harvard University Press, 1934. $4.50.

The Unity of Science, by Rudolf Carnap. Kegan Paul, 1934. 2/6.

Das Experiment und die Metaphysik, by Edgar Wind. Verlag von J. C. B. Mohr. Tübingen, 1934.

The Sub-Atoms, by William Mayo Venable. The Williams & Wilkins Co. $2.00.

Non-Euclidean Geometry or Three Moons in Mathesis, by H. G. L. R. Lieber. Academy Press, New York.

Galois and the Theory of Groups, by H. G. L. R. Lieber. Academy Press, New York.

ANNOUNCEMENT

An informal meeting of persons interested in the advancement of logical studies, and particularly in the possibility of providing greater facilities for the publication of papers in symbolic or mathematical logic, was held on December 27th at New York University during the annual meeting of the Eastern Division of the American Philosophical Association.

A Committee of Organization consisting of the following persons was appointed:

C. A. Baylis—Philosophy, Brown
A. Church—Mathematics, Princeton
M. R. Cohen—Philosophy, C. C. N. Y.
H. B. Curry—Mathematics, Penn. State
C. J. Ducasse—Philosophy, Brown
S. K. Langer—Philosophy, Radcliffe
J. B. Rosser—Mathematics, Princeton
H. B. Smith—Philosophy, University of Pa.
P. Weiss—Philosophy, Bryn Mawr.

Since more definite plans for the proposed Journal must depend largely on the funds that would be available, all persons (whether or not themselves working in the field of Logic) who would favor the launching of such a Journal, and who would be willing to lend it their support by joining the proposed association (with dues of probably $3 a year) are asked to send their names to Professor C. J. Ducasse, Brown University, Providence, R. I.

A NEW BUDGET OF PARADOXES

(5) NOTES FROM A CASE-BOOK

Case 5. Poisoner, sub-class glossifiers. Still kicking and selling. Characteristic utterance: "Chemists of America attend! The further progress of science and philosophy urgently requires that the error of experiment and fallacy of interpretation made by Lavoisier in his supposed overthrow of the phlogiston theory of combustion shall now be rectified." The error was but by one five-billionth of a part. What is wrong with us—"the destructive chaos of a fortuitous and syncretic materialism;" what is right—"Big Education, Big Politics and Big Business"! The organization back of this is none other than the Philadelphia School of the University of the World, Instituted to Propagate the Philosophy of Americanism.

Case 6. Magician, paladin of the balance. Two contributions entitled "Fundamental Facts" and "Origin of the Elements." The universe is constructed out of simple bricks, to wit the numbers 0.7854, 0.6073, 0.5236, 0.4764. It is remarkable what a series of divisions and multiplications can do to these. God is a mathematician. Also "experiment has shown that the heat or force necessary to decompose water, would raise it to a height of 5,314,200 feet. This is only 210 feet less than 10 times the square of 729, which is the cube of 9. This shows that the foot as a measure of length is very nearly exact and that the yard is more appropriate than the Meter. Another remarkable fact is that after 531,441 is divided by 1,000, the square root is found to be 23.053004. This is very nearly the combining proportion of sodium. The true number is $14 \div .6073 = 23.05282$." Apparently errors of one five-billionth are still possible. And, incidentally, we must not forget that "the heart is on the left side because the sun and the planets rotate and circulate from right to left while they descend."

Case 7. Overman, sub-class god. The author believes that man can speak with God (with a capital "G"). The message the Galilean Carpenter gave to this earth, 2000 years ago, HAS BEEN MISSED IN ITS ENTIRETY. Revelation came in a drug store out West, and a movement was floated on $40.—"destined to become the future Religion of America." Hints are given that the cosmic rays are the true spiritual beams, but you can't get the real secret unless you send that dollar.